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THE PSYCHOLOGICAL REVIEW

FACTORS DECISIVE FOR RESUMPTION OF INTERRUPTED ACTIVITIES: THE QUESTION REOPENED [1]

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The question of the factors upon which resumption of interrupted activities depends has received surprisingly little attention despite the importance of the interruption technique for the experimental study of human motivation. Two possible explanations of the resumption of unfinished tasks have been proposed by Ovsiankina (5, p. 340), further investigated by Adler and Kounin (1), and subsequently discussed by Lewin (3, p. 820). These hypotheses define the problem with which the present paper is concerned:

1. The decisive factor for resumption might be the need tension created when the subject originally undertook to perform the task. The incompleteness of the task itself, then, would only be significant because the corresponding need remained unsatisfied. Resumption in this case would have the function of resolving the underlying need tension.

2. Resumption might arise primarily from the subject's perception of the incompleteness of the task itself and his recognition that the task requires action. This perception might be assumed to arouse a need for completion in the person. But the function of the resumption would be to meet the demands of the task, not primarily—or, at least, not originally—to satisfy the needs of the individual.

Both hypotheses, it will be noted, assign a role in resumption to the subject's need and to the incompleteness of the task, which possesses a valence for completion. But the first alternative makes the valence of the unfinished work a mere symptom of the already existing need tension, whereas the second hypothesis states that the forces responsible for resumption may issue primarily from the perception of the unfinished task itself. The two hypotheses are, of course, not mutually exclusive.

It might be argued that since we must assume that the needs of the person are involved in both cases, no real theoretical issue exists. But this does not solve the problem: the problem merely repeats itself on a different level. For then we must distinguish between the ego's needs in the service of a situation and those in the service of the individual (cf. 2).

There seems to be ample evidence that an unsatisfied need of the subject may lead to the resumption of an interrupted activity. Ovsiankina's result, a very high percentage of resumption of interrupted activities, was obtained with tasks which were trivial in nature, which

[1] This paper was prepared by Mary Henle when she was a Fellow of the John Simon Guggenheim Memorial Foundation.

did not urgently demand action from the subject. It was obtained, furthermore, with a variety of tasks, whose specific demands varied. There would appear, then, to be little doubt that a need tension within the subject may be decisive for resumption.

On the other hand, the evidence now to be reviewed would seem to exclude the second hypothesis.

Ovsiankina reports two sets of observations which lead her to reject or minimize the perceived incompleteness of the task as a factor decisive for its resumption.

1. She studied the part which the actual sight of the incomplete task played in 230 cases of resumption. In only a minority of cases (22 per cent) was resumption clearly related to the sight of the unfinished task. In a larger number of cases (33.5 per cent) the previous sight of the task was not necessary for resumption, as evidenced by subjects' introspections; also by instances in which the incomplete task was purposely hidden from the subject, who then had to ask for it or search for it in order to resume it; and by cases in which the subject, while in another room, announced his intention of resuming the unfinished work. The remaining instances were transitional cases in which, Ovsiankina states, it was difficult to decide just how much the sight of the task had to do with the resumption.

2. Ovsiankina undertook in another manner to decide whether the needs of the subject or the demands of the task are decisive for the resumption of unfinished activities. Preliminary experiments were performed to discover whether resumptions would occur with unfinished tasks which had not originally been started by the subject himself and for which, therefore, no need tensions could be assumed.

Two tasks which had been used successfully in previous experiments were employed. They were left partially completed on a laboratory table. Only 3 of 11 subjects (27 per cent) resumed the unfinished work, although the same tasks showed resumption in 79 per cent of the cases in the main experiments with the interruption technique. By contrast with the prompt resumptions in the main experiments, one of the three resumptions occurred only after 40 minutes in the experimental room.

A repetition of the experiment with a different, more familiar task gave substantially the same results; only 1 of 11 subjects (9 per cent) resumed the incomplete task which was not his own, whereas the same activity had been resumed in 82 per cent of the cases in experiments with the interruption technique.

From the above evidence Ovsiankina comes to the following conclusions (5, pp. 345–6):

> A tendency to completion may issue from an incomplete activity. In our experiments, however, this tendency is extraordinarily weak by comparison with the forces driving to resumption which issue from the quasi-need arising from the interruption of work which has been begun. *The decisive factor, therefore, for the tendency to resumption in our experiments is the existence of a corresponding inner tension system.*
>
> Such an inner tension does not arise from the subject's perception of an incomplete task which is not his own, but presupposes the establishment, by instruction or occupation with the activity, of a special relationship between the subject and the work such that the person makes the work *his own.*

The last described experiment of Ovsiankina has been criticized by Adler and Kounin (1) in a number of respects:

1. The number of subjects employed was insufficient.

2. No attempt was made to ascertain that the incomplete task started by an-

other person was actually psychologically present in the subject's life space.

3. In the absence of specific instructions, the subject had no reason to believe that completion of the task was not prohibited.

Adler and Kounin devised experiments which are free of these difficulties and which they consider, therefore, to be more suitable for deciding whether personal need or perception of the incompleteness of the task is decisive for the resumption of interrupted activities. Their experiments were intended to create a situation in which two identical unfinished tasks were present in the subject's life space at equal distances from him. One of the tasks had been started by the subject himself ("I-task" or interrupted task), while the other was presented in a condition of incompleteness ("U-task" or unfinished task). Thus a quasi-need could be assumed only for the former. Should the "own" task be chosen for completion, Adler and Kounin state, this preference must be ascribed to the effect of the quasi-need within the person. If, on the other hand, no marked difference should be found in the amount of resumption of the two tasks then, Adler and Kounin propose, the need within the person must be eliminated as a decisive factor, and resumption would have to be accounted for by reference to the perception of the incomplete task alone.

The experiment of Adler and Kounin was performed with 22 children, ranging in age from about four to five years. Twenty subjects or 91 per cent resumed the own task (I-task), one subject (4 per cent) resumed the task started by another person (U-task), and another subject failed to resume either of the tasks. The resumptions occurred promptly and were generally followed by completion of the task.

According to the authors (1, p. 263) these findings all argue for the same conclusion—resumption of the interrupted task depends here upon the established tension system corresponding to the quasi-need. The resumption of the U-task in only one case out of twenty-two argues against an interpretation of this resumption based upon the nature of the unfinished task, per se. Generalizing from these data we may say that the tension t(G) is a determining factor in resumption.[2]

These conclusions are in essential agreement with those of Ovsiankina cited above.

Lewin (3, p. 820) cites the work which has been described here as evidence that "the presence of uncompleted work of another person does not lead (or extremely seldom leads) to spontaneous completion in adults (Ovsiankina, 1928) or in children (Adler and Kounin, 1939). Both results indicate that the state of the need of the child is decisive for resumption."

While there are undoubtedly many cases which fit this description, a number of considerations make us question the general validity of these conclusions:

1. It is of interest to note that the conclusions of Ovsiankina, Adler and Kounin, and Lewin which have been cited here are not consistent with the general theoretical framework from which the studies in question evolved. Behavior, Lewin states, is a function of both the person and the psychological environment. He regards behavior as truly the result of an interaction between person and environment. But interaction cannot be regarded as a one-way affair, the person simply acting on a passive environment. We should therefore expect to find cases in which the forces underlying behavior issue primarily from the perceived object in the environment, and not only cases in

[2] "t(G) = the tension corresponding to some quasi-need . . ." (1, p. 257).

which the forces arise primarily in the person.

2. The conclusions drawn by Lewin, Ovsiankina, and Adler and Kounin are in conflict with certain evidence from real life situations. We know that there are situations which call for action and which are acted upon by observers who have had no previous part in the event. For example, we right a chair which is about to fall or prevent an accident by last-minute action. There are also cases in which people do resume the unfinished tasks of others, for example resuming and carrying forward research which leaves essential issues undecided, taking up and following through the ideas and ideals of others. The same applies to many situations which involve helping and replacing others.

3. Experimental evidence which appears in certain respects to contradict the conclusions of Adler and Kounin has been presented by Lewis (4). This investigator has demonstrated that in a truly cooperative situation the completion of a task by a co-worker may be just as satisfactory as completing the task oneself. More specifically, Lewis has shown that in a cooperative situation no difference exists in the amount of recall of self-completed tasks and interrupted tasks which the partner has completed. Zeigarnik (7), it will be recalled, demonstrated that unfinished tasks are favored in memory over completed ones. In Lewis' experiment, tasks left unfinished by the subject but completed by a cooperating partner behaved like completed tasks in recall.

Since these findings are opposed to the conclusions of Adler and Kounin, Lewis is led to call these conclusions into question. She writes (4, p. 206): "Actually, this experiment of Adler and Kounin missed the crucial point. Their results do not demonstrate that a person's needs may not directly conform to the objective requirements of the situation, but only that when the objective requirements are *identical*, a greater need arises to complete a task which one has personally begun."

Lewis' criticism may be warranted, but an even more fundamental difficulty has hitherto been overlooked. When comparing the effects of two conditions (here "ownership" vs. nonownership of a task), one assumes that the psychological situation is the same except for the condition being varied. The investigation of Adler and Kounin makes this assumption. These experimenters offered their subjects the choice between objectively identical I- and U-tasks for resumption. Their investigation rests on the assumption that these tasks were likewise *psychologically* identical for the subjects. This is something that must be established, however, rather than taken for granted. Once we consider the psychological situation, it is no longer self-evident that the two alternatives were alike in all respects except ownership. Before the argument of Adler and Kounin can be accepted, therefore, investigation is needed to determine the meaning of the I- and U-tasks for the subjects. In the absence of such investigation we are not justified in drawing conclusions about the factors responsible for the resumption of activities.

EXPERIMENTAL OBSERVATIONS

The first problem of investigation consists, therefore, in a re-examination of the work of Adler and Kounin. In order to do this, we report briefly a repetition of the experiment of these writers. The only important respect in which the present experiment differs from that of Adler and Kounin is that an effort was made to learn as much as possible about the subject's understanding of the experimental situation. Otherwise the experimental plan of the previous investigators was followed:

Two identical tasks, one started by the subject himself (I-task) and one left unfinished by someone else (U-task), were offered to the subject for resumption.

Experimental tasks. The tasks employed in this experiment were similar to those of Adler and Kounin. The I- and U-tasks included two identical colored cardboard houses, 10 inches in length by 6 inches in width. Before each house was a green wooden pegboard, 13 by 8 inches, each containing 192 holes. About 50 colored wooden pickets were provided with each house; these were to be fitted into the holes outlining the "yard" so as to construct a fence. In addition, some 30 colored pegs of various shapes, to be used as trees, bushes, flowers, etc. were supplied for each house and were to be used to make a garden. These materials were set out on a table, about 81 by 40 inches, in such a manner that the two tasks were about 40 inches apart.

The interruption task consisted of a small oilcloth duck partially stuffed with cotton wool. Additional stuffing was provided, and the child's task was to stuff the duck and pin it together.

Procedure. The materials for the I- and U-tasks were set out on the experimental table before the experiment began. The former (I-task) was left unstarted, all the pegs still contained in their boxes. The U-task had already been begun: about ten pickets had been placed into holes to start the fence, and one tree and two flowers had been set out in the garden. The *E* brought *S* into the experimental room and led him to the experimental table. The child was allowed a moment in which to survey the situation. Then *E* gave the following instructions:

Look, Jimmy, what we have. Now I am going to tell you all about it. You see the two houses? (*E* leads *S* to the U-task.) This house has some trees and flowers in the garden and it has part of a fence around the yard. (*E* leads *S* to the I-task.) Now here is the other house. But it doesn't have a garden yet and no trees and flowers and no fence. And here in these boxes are all the things that you need for a fence and a garden. Now would you like to make a fence all around the house and make a pretty yard with all these trees and flowers?

When the I-task had been brought to approximately the same degree of completion as the U-task, *E* interrupted *S* by saying:

Come over here, please, and see what I have here. Look at this nice duck. It's all flat; the cotton came out. I would like you to put some cotton in and make him nice and fat again.

When *S* had finished this task, *E* had intended to say: "Now, Jimmy, I have some writing to do. You go ahead and play by yourself with anything you want." These last instructions, however, never had to be used in this experiment.[3]

Careful records were kept of *S*'s behavior during the experiment and of all relevant spontaneous remarks. In addition, questions were occasionally asked by *E* in order to appraise the psychological situation of the subject.

Subjects. Twenty children, ten boys and ten girls, served as subjects in this experiment. They were pupils in a kindergarten class at the Gregory Avenue Public School in West Orange, N. J. They ranged in age from five to six years. Their IQ's ranged from 89 to

[3] Our experimental procedure, it should be noted, meets the conditions which Adler and Kounin consider to be necessary if the experiment is to provide a real test of the strength of the tendency to resume one's own task as compared with the work of another (1, p. 260): (*a*) To insure the psychological presence of both tasks for *S*, *E* introduced him to the U-task before allowing him to proceed with the I-task—the same method employed

140, with a mean of 107. *E* had become acquainted with the children during a two-hour session prior to the experiments.

Results. All 20 children resumed their own task after completing the interruption task. Our results thus offer striking confirmation of the quantitative findings of Adler and Kounin.

All resumptions occurred immediately and spontaneously; indeed, so prompt were the resumptions that *E* never had the opportunity to give the last part of the instructions. Noteworthy also was the marked reluctance of subjects to interrupt the I-task; repeated urging was generally needed. All *S*s, furthermore, carried the I-task to completion.

The interpretation of Adler and Kounin, as stated above, rests on the assumption of the psychological identity of the I-task and the U-task. Specifically, it is assumed (*a*) that the two tasks are psychologically identical for the child, and (*b*) that they are psychologically on equal terms. Our qualitative data, now to be presented, challenge both these assumptions.

1. The qualitative evidence clearly shows that the two tasks are not psychologically identical. The child, in the course of working with the I-task, gives it a specific meaning or a new function which the U-task lacks. The I-task, in other words, comes to acquire new properties not possessed by the U-task. The demands of the two activities can thus no longer be considered to be alike. Two types of protocols may be cited as evidence for this difference.

a. A number of subjects made up stories around the I-task. Completion, then, no longer meant merely meeting the formal demands of the task as envisaged by *E* (demands also possessed by the U-task); the demands of the story, too, had to be satisfied. The following protocols are representative:

Subject 1: "I better hurry up or the dog will run out. . . . Their Great Dane will run out if I don't hurry." (Finishes fence.) "There, now you can't get out!"

Subject 4: "I'm going to make it *real* pretty. . . . This house is for sale. That's why it has to be pretty."

Subject 6: "I am going to put a lot of flowers in. . . . What are we going to do? The children will pick all the nice flowers. The mother just will have to put another fence around so they can't pick them. Just wait, children, in half an hour I will be done. Let's hurry, or they will pick them on the other side. . . . So, now they can't pick the flowers."

In these cases, then, the task is no longer the task as it was set by *E*. It is not just a matter of building a fence around a house; rather, the Great Dane must be prevented from running away or the flowers must be protected against children, or else a house must be made suitable for sale. These specific meanings and requirements are not possessed by the U-task. The I-task is thus not psychologically equivalent to the U-task. Its properties and requirements are different and, one also suspects, more compelling than those of the U-task.

b. Other *S*s, prior to interruption, announced a plan of action with regard to the I-task.[4] Completion of the latter

by Adler and Kounin. (*b*) The final instructions, modeled after those of the previous investigators, were intended to make *S* feel perfectly free to resume either task after interruption or not to resume at all. Striking evidence of the freedom of the experimental situation is the fact that these additional instructions were never needed. (*c*) That the tasks possessed intrinsic appeal for our *S*s was evidenced throughout by their behavior.

[4] This possibility has been mentioned by Torrey (6, p. 203). Commenting on the interpretation of Adler and Kounin, she writes: "The perception of a task which is merely seen partly done by another person and that of a task which the individual has himself worked on are themselves sufficiently different to warrant the prediction that they will in-

thus means the specific completion demanded by the plan. Again the properties of the I-task have changed as compared with those of the U-task. The following examples may be cited:

Subject 11: "I am going to use pretty colors and put trees right across the front."

Subject 19: "I want this tree right here; I like it. There is nothing going to be here, except all little flowers. My aunt has a house like this."

2. In a considerable number of cases, the qualitative evidence reveals, the I- and U-tasks come to be distinguished by the different roles which they play in the experimental situation. The U-task is perceived as a model, while the I-task becomes the main task.

a. Several *Ss* expressly stated their belief that the U-task was introduced as a model. For example:

Subject 16: "I am going to make it (fence) all around, better than this" (pointing at U-task).

Subject 18, after staring at the U-task in fascination, happily starts his I-task. Later, pointing to the U-task, he remarks: "I bet you this (U-task) is to show how to do it and this (I-task) is to do. Right?"

b. Other *Ss* clearly indicated by their behavior during the experiment, though they did not expressly state it, that they considered and used the U-task as a model for the I-task. They studied the U-task before starting their own, walked over to it while they were doing the I-task, ran back and forth between the two tasks or looked over to the U-task before making new moves on the I-task. This was particularly true of *Ss* who otherwise displayed insecurity in their behavior. The following examples are typical:

Subject 2, a frightened little girl, kept looking back and forth between the U-task and her own, anxiously using the former as a model.

Subject 10, after finishing the fence, walks over to the U-task, her hands full of pegs, and looks at it again before she starts to work on the garden.

Subject 17, though evidently anxious to start the I-task, runs over twice to inspect the U-task before he begins work on his own.

Thus, by assuming the role of model, the U-task becomes psychologically different from the I-task. As with our other cases, one suspects that here too the difference between the two activities favors the resumption of the I-task. It seems safe to assume that the forces issuing from the main task are stronger than those issuing from a mere model, which is hardly a task at all in its own right.

Almost all *Ss* of this experiment can be placed in one or more of the categories listed above. In a few cases the child was not sufficiently communicative and his behavior not sufficiently revealing to indicate the specific meaning of the experimental tasks to him. There seems, however, to be no reason to assume that the psychological situation was essentially different for these uncommunicative children than for the others.

Since the procedure of the present experiment was patterned closely after that of Adler and Kounin, and since the quantitative results agree well with theirs, it may be assumed that the essential conditions of these investigators have been reproduced here. But qualita-

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fluence behavior differently. . . . Perhaps the well-formulated plan of how the task shall be done is the most important additional aspect of the task actually begun *by* the subject. The tension when this plan, which is the product of the subject's own imagination, is broken into would be much greater than what might be inspired by mere inspection of someone else's half-done work."

The present experiment, which was completed before the publication of Torrey's paper, shows that this is a good hypothesis, though not sufficient to account for the results of Adler and Kounin.

tive study reveals that under these conditions the I-task and the U-task, despite their physical identity, are by no means psychologically identical.[5] Furthermore, the differences between them appear to be such as to favor the resumption of the I-task. We can therefore not conclude, as do Adler and Kounin, that this task is preferred to the U-task because it is the individual's own, not someone else's activity. It might also be that the other differences in the properties and roles of the two kinds of tasks are responsible for the difference in resumption between them.

The question of what factor is decisive for the resumption of interrupted activities—the tension system within the person or the perceived demands of the task—is thus reopened by the findings of the present experiment. We are again led to ask: Will it always be true that a person deals with his own task in a privileged way? We have preliminary evidence which suggests that this is not always the case, that there are conditions under which a child is just as likely to resume and carry to completion the unfinished activity of another person as to start a task of his own. In any case, it is clear from the present investigation that it is by no means established that the needs of the individual are the only factors decisive for resumption.

[5] It might be argued that our experiment has only shown that, in the course of the individual's relations with an object, changes in the latter occur. This argument does not, however, do away with our present problem: we are here concerned with testing the assumption of Adler and Kounin that the properties of the I-task and the U-task are identical, so that a preference for the former in resumption is to be attributed to the quasi-need of the subject. The error of these investigators consists precisely in ignoring the changes which occur in a task in the course of the individual's relations with it.

Since the relation between the subject's own need and his resumption of an interrupted activity seems to have been amply demonstrated (though not by Adler and Kounin), the first task of future investigation in this area would seem to be to create situations in which the perceived requirements of the task determine resumption. Such investigation would be of significance for an understanding of the nature of the relation between the individual and his psychological environment as well as for such more specific problems as the present one of the factors determining resumption or the question of cooperative action.

The methodological implications of this study are clear. In the present field of investigation quantitative findings in the absence of qualitative study of the individual's understanding of his situation are likely to be misleading.

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THE MEANING–FAMILIARITY RELATIONSHIP [1]

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The present series of investigations represents an attempt to identify and quantify some of the attributes of verbal material relevant to human learning. The first paper of this series (10) reported an analysis of stimulus meaning (*m*). Evidence reviewed in McGeoch (7), Underwood (17), and Woodworth (18) indicates that the *familiarity* of a stimulus or a response item may also be an important variable operating in verbal learning. Some psychologists, however, have questioned whether meaning and familiarity are distinct concepts. Underwood (17), for example, suggests that "meaningfulness . . . is the same characteristic as implied by the term *familiarity*" (p. 411). Others (5, 9, 18) have indicated only that some positive correlation may obtain between these two variables.

The objectives of the present study are (*a*) an analysis of the attribute of familiarity in verbal stimulus material, and (*b*) an investigation of the functional relationship between meaning and familiarity.

An Analysis of Familiarity

Theoretically, the familiarity of a stimulus (or response) may be regarded as some function of its frequency of occurrence in an organism's history. Humans may experience familiarity with a verbal stimulus in a number of ways: through the special senses of vision or audition, by means of speech (spoken or implicit), or by writing. By these means familiarity may become a learned stimulus attribute. The learning process is at least descriptively clear. Frequency of stimulation—including that which is response-produced, or proprioceptive—is a necessary condition of learning. That it is not also a sufficient condition is attested to by the research reviewed by McGeoch (7). For verbal learning, as in many other types, some kind of reinforcement or knowledge of results seems jointly necessary.

Stimulus frequency is a variable to which a judge may be motivated and set to respond differentially. The empirical procedure of eliciting such differential responses might be by requiring *S* to point to a list of stimuli singly to indicate their degrees of *familiarity* to him. Important work related to this concept has been done by Thorndike (14), Thorndike and Lorge (15), and by Haagen (5). In brief, a consideration of the attribute of familiarity (*f*) reveals that it is a concept definable by the operations involved in a psychological (or psychometric) scaling method.

Assuming Thurstone's (16) law of comparative judgment, and utilizing the method of successive intervals developed by Saffir (12), Mosier (8), and Edwards (3), one may transform frequencies of rating into deviates of the normal curve to yield a set of scale values having the properties of an equal-interval scale. Hence, the familiarity of any stimulus, *S*, is given by

$$f_s \equiv l + \left(\frac{.50 - cp}{p}\right) i, \qquad [1]$$

[1] The opinions or conclusions contained in this report are those of the author. They are not to be construed as reflecting the views or endorsement of the Department of the Air Force.

where $l \equiv$ lower limit of interval containing Mdn in x/σ units; $cp \equiv$ cumulative proportion below interval containing Mdn; $p \equiv$ proportion within interval containing Mdn; $i \equiv$ interval width on familiarity continuum in x/σ units.

Procedure

The same list of 96 dissyllables used in establishing the author's *m*-scale (10) was used to determine quantitative values for the attribute of stimulus familiarity. It was decided that a graphic rating schedule (method of single stimuli) was the most suitable defining task for this purpose. More elaborate methods, such as that of paired comparisons, would have been laborious and time consuming considering the number of stimuli involved. A rank-order method would have been inappropriate in view of the nature of the attribute investigated, since the act of ranking might have seriously altered the original values of the items. In addition, recent advances in the theory of measurement have provided a rationale and an internal consistency check for the present method. Finally, it was believed that the sample of 200 *S*s was adequate to insure a high reliability for the ratings.

The stimulus items were administered in mimeographed test booklet form. Twelve items appeared on each page, followed by spaces for graphic ratings arranged in columnar form. The complete list sequence was randomized with the aid of a table of random numbers, and the sequence and order of pages were counterbalanced by shuffling the attached answer sheets prior to administration. The sample totaled 200 airmen, selected from four different flights undergoing routine classification testing at the Human Resources Research Center. The *S*s were group-tested in four units of 50 men each, and the testing periods were held during August 1951. A test session was of approximately 20-min. duration. Instructions to the *S*s were as follows:

This is a test to find out how often you have come in contact with certain words.

You will be given a list of 96 nouns and you are to rate each one as to the number of times you have experienced it by simply placing a check mark (√) in one of the five spaces provided for your rating.

The five possible ratings are described by the words NEVER, RARELY, SOMETIMES, OFTEN, and VERY OFTEN. This means that you have *seen* or *heard* or *used* the particular word (in writing or in speech) either:

1. NEVER (You have *never* seen or heard or used the word in your life.)

or

2. RARELY (You have seen or heard or used the word at least once before, but only *rarely*.)

or

3. SOMETIMES (You have *sometimes* seen or heard or used the word, but not often.)

or

4. OFTEN (You have *often* seen or heard or used the word, but not very often.)

or

5. VERY OFTEN (You have seen or heard or used the word *nearly every day* of your life.)

Do not be bothered if you are unable to give a definition of some of the words. Simply rate each one as to the number of times you have come in contact with it regardless of its meaning.

There may be some words which you have *used* or *heard* more often than you have *seen* them. Or there may be other words which you have *seen* more often than you have *used* or *heard* them. In such cases, always give the word the *highest* rating of the three.

For example, you probably use or hear the word FLIGHT CHIEF often, but you may never have seen it in print. In this case, you would rate FLIGHT CHIEF as "Often," as shown on the line below:

FLIGHT CHIEF:

Never	Rarely	Sometimes	Often	Very Often
			√	

The *S*s were then given four practice tasks, using as stimuli the words BREAKFAST, ZITHER, ENGLAND, and SYZYGY.

Instructions regarding motivation and set were as follows:

When the examiner gives you the "GO" signal you will turn the page and begin rating the list of words at your own speed. This is not a "speed" test, for each man will be given plenty of time to finish. The important thing is for you to be as accurate as possible.

Be as honest in your ratings as you can. Many of the words in this test are very rare, so you are not expected to have come in contact with all of them. Just make the best estimates you are capable of.

Pay no attention to those working near you, for the words are in mixed order and each man will be working on a different item.

Results and Discussion

The graphic method of recording *S*'s responses was simple, objective, and rapid. The index of familiarity (f) of a particular stimulus was defined in equation [1] as the median of the distribution of judgments on the familiarity continuum. Therefore, the method of successive intervals outlined by Edwards (3) was followed to compute the interval widths of the middle three categories. Failures of rating were not counted, the n's of the corresponding arrays being decreased accordingly. Pro-

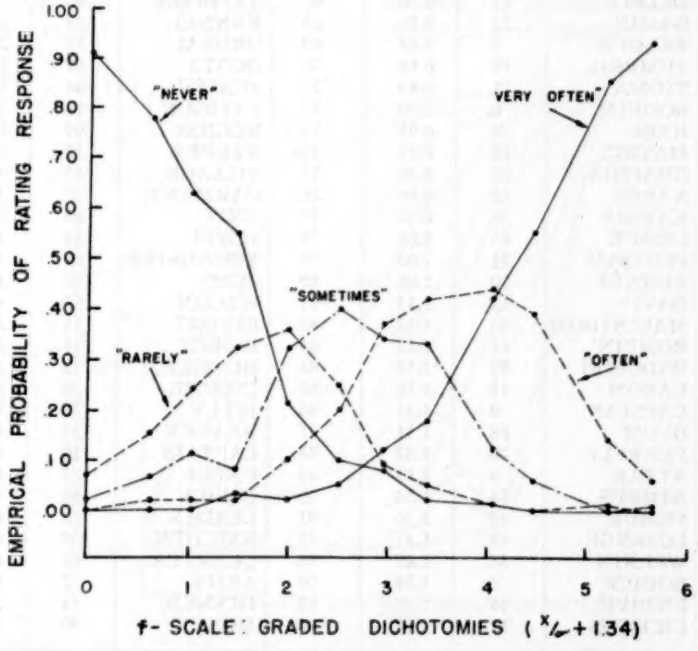

FIG. 1. Relative frequency distributions of graphic ratings in the five categories of the Familiarity Schedule for 12 representative stimulus words as functions of f-value derived from the method of graded dichotomies ($N = 200$)

TABLE I

LIST OF DISSYLLABLE WORDS (NOUNS) IN RANK ORDER OF INCREASING FAMILIARITY (*f*) AS DEFINED BY NORMAL DEVIATE TRANSFORMATIONS OF RATINGS ON FIVE-POINT GRAPHIC RATING SCHEDULE ($N = 200$)

Rank	Word	Word No.	*f* Value	Rank	Word	Word No.	*f* Value
1	POLEF	63	0.00	49	TANKARD	76	1.71
2	NOSTAW	55	0.10	50	NAPHTHA	51	1.73
3	GOKEM	25	0.16	51	TARTAN	78	1.85
4	LATUK	41	0.16	52	ROSTRUM	72	1.93
5	VOLVAP	86	0.17	53	ARGON	1	2.00
6	ZUMAP	96	0.19	54	YEOMAN	91	2.15
7	GOJEY	24	0.21	55	JITNEY	35	2.17
8	BYSSUS	8	0.39	56	VERTEX	84	2.17
9	XYLEM	90	0.40	57	RAMPART	68	2.21
10	SAGROLE	73	0.42	58	PERCEPT	61	2.30
11	TAROP	77	0.62	59	PALLOR	60	2.33
12	QUIPSON	66	0.64	60	PALLET	59	2.52
13	KUPOD	40	0.66	61	SEQUENCE	74	2.54
14	JETSAM	33	0.67	62	QUARRY	64	2.55
15	ULNA	81	0.67	63	ENTRANT	17	2.60
16	NEGLAN	53	0.69	64	MALLET	47	2.67
17	MEARDON	49	0.73	65	PIGMENT	62	2.67
18	BALAP	4	0.74	66	ZEBRA	93	2.74
19	DELPIN	13	0.76	67	TYPHOON	80	2.81
20	GAMIN	22	0.79	68	KENNEL	38	2.82
21	BRUGEN	7	0.85	69	ORDEAL	57	2.84
22	TUMBRIL	79	0.86	70	QUOTA	67	2.97
23	STOMA	75	0.89	71	ZENITH	94	2.98
24	BODKIN	6	0.90	72	FATIGUE	18	3.18
25	ICON	29	0.91	73	REGION	69	3.40
26	MATRIX	48	0.92	74	KEEPER	37	3.58
27	GRAPNEL	26	0.94	75	VILLAGE	85	3.67
28	NARES	52	0.96	76	GARMENT	23	3.69
29	KAYSEN	36	0.97	77	UNIT	83	3.70
30	LEMUR	43	1.01	78	JEWEL	34	3.71
31	FLOTSAM	21	1.02	79	YOUNGSTER	92	3.85
32	RENNET	70	1.06	80	ZERO	95	3.85
33	DAVIT	12	1.13	81	WAGON	87	3.93
34	MAELSTROM	46	1.13	82	EFFORT	15	3.96
35	ROMPIN	71	1.13	83	INSECT	31	3.97
36	WIDGEON	89	1.14	84	HUNGER	28	3.98
37	CAROM	11	1.18	85	INCOME	30	4.06
38	CAPSTAN	9	1.31	86	JELLY	32	4.11
39	OVUM	58	1.31	87	HEAVEN	27	4.19
40	FERRULE	20	1.32	88	CAPTAIN	10	4.27
41	ATTAR	3	1.33	89	UNCLE	82	4.34
42	NIMBUS	54	1.34	90	OFFICE	56	4.44
43	FEMUR	19	1.36	91	LEADER	42	4.47
44	LOZENGE	45	1.37	92	KITCHEN	39	4.71
45	WELKIN	88	1.49	93	QUARTER	65	4.76
46	BODICE	5	1.58	94	ARMY	2	4.79
47	ENDIVE	16	1.59	95	DINNER	14	5.22
48	LICHENS	44	1.67	96	MONEY	50	5.66

portions ≥ 0.98 and ≤ 0.02 were arbitrarily rejected, as is commonly done in the method of paired comparisons (4).

In view of the fact that for 51 of the 96 items more than 50 per cent of the judgments fell in either of the extreme categories, thereby constituting indeterminate successive intervals *f*-values, Attneave's (1) method of graded dichotomies was adopted to estimate the medians. These *f*-values are shown in Table 1, together with their reference numbers, ranging in rank order from dissyllables of low familiarity (e.g., rank 1: POLEF) to those of high familiarity (e.g., rank 96: MONEY). In order to orient the scale to an arbitrary origin, a constant of 1.34 was added to all values. The empirical range in x/σ units was then from 0.00 to 5.66.

One advantage of the method of successive intervals (or graded dichotomies) lies in the test of internal consistency which it affords. By using the $N + r - 2 = 99$ parameters one can reproduce the $N(r - 1) = 384$ independent empirical proportions with an average error of 0.019.

Further evidence related to the internal characteristics of the *f*-scale is exhibited in Fig. 1. This graph plots the relative frequency distributions of the judgments in the five categories of the Familiarity Schedule for 12 stimulus words as functions of *f*-value. The 12 items were selected to represent approximately equal intervals along the scale. The regularity and symmetry of the probability functions is apparent.

Results very similar to these were obtained with a preliminary sample of 100 airmen in 1950. The rating schedule differed in that a category of DOUBTFUL was used instead of the RARELY category, but otherwise the stimuli and procedure were identical. The average

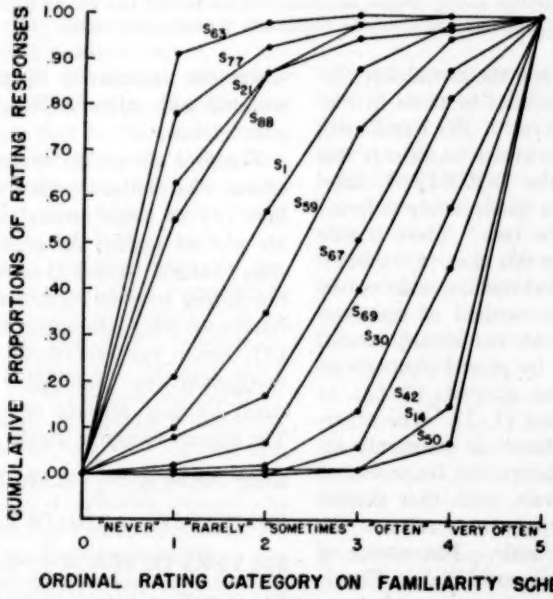

FIG. 2. Cumulative proportions of ratings in the five categories of the Familiarity Schedule for 12 representative stimulus words ($N = 200$)

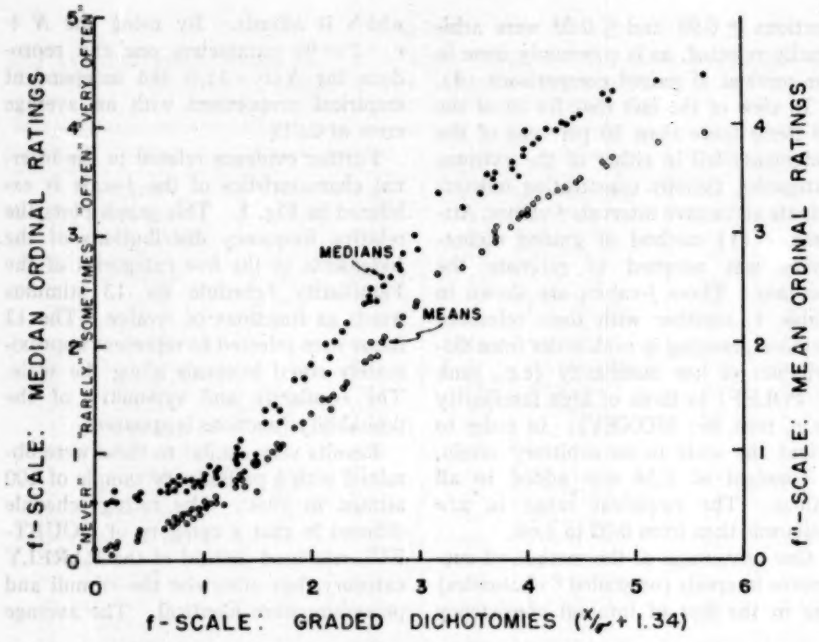

FIG. 3. The relationships among median and mean ordinal ratings and graded dichotomies scale values derived from the Familiarity Schedule ($N = 200$)

error was 0.023, and the probability distributions were similar to those in Fig. 1. This early form of the Familiarity Schedule was discarded because it was suspected that the DOUBTFUL category introduced a qualitatively different judgment into the task. There is now little evidence for this view ($r = 0.992$).

It is known that median scale values derived from the method of equal-appearing intervals are not linearly related to those treated by paired comparisons (6, 12), successive intervals (3, 12), or graded dichotomies (1, 3). The occurrence of "end-effects" is commonly attributed to the behavioral *inequality* of the rating intervals, such that skewed distributions of judgments arise at the extremes of the scale. The source of this nonlinearity is exhibited in Fig. 2. Here the skewness is seen to decrease from both ends toward the midpoint, where the cumulative distribution associated with stimulus No. 59 is quite symmetrical.

Figure 3 shows the consequent end-effects when either median ordinal ratings (f') or mean ordinal ratings (f'') are plotted against the graded dichotomies f-values. It should be recalled that the judges were not instructed to form equal-*appearing* intervals as is often done (4), hence the end-effects in this investigation are presumably uncomplicated by any artifact of instructions. The average category widths in x/σ units were: NEVER $= \frac{0}{0}$; RARELY $= 0.82$; SOMETIMES $= 0.90$; OFTEN $= 1.12$; and VERY OFTEN $= \frac{0}{0}$. Inspection of Fig. 3 indicates that, contrary to popular opinion (4), mean ratings are at-

tended by somewhat less distortion than are medians.

In order to satisfy the second objective of this study, the determination of the functional relationship between meaning and familiarity, a necessary condition is the possession of equal-interval scales for both variables. For the status of the *m*-scale, see (10). Since the logic of paired comparisons underlies the present scaling method, and since paired comparisons transformations are linearly related to those of successive intervals (3, 12) and to graded dichotomies (1, 3), then the unit-distance condition is satisfied for the *f*-scale.

THE MEANING-FAMILIARITY RELATIONSHIP

Several writers have called attention to the relationship between the frequency of appearance in writing and the number of synonyms of words in the English language. Zipf (19) and Thorndike (14) have shown this correlation to be a positive one.

With respect to another correlate of frequency—familiarity—certain psychologists hold that its relation to meaning is one of *identity*. Of course, the verification of this hypothesis is contingent upon variables which traditionally have been left undefined.

From logical considerations, the author prefers to regard any hypothetical meaning-familiarity relationship not as an identity, but rather as a determinate correlation between two different attributes. An *outré* example will serve to clarify the logic underlying this expectation. Consider an *S–R* connection which has been reinforced fifty times; that is to say, a stimulus element, S_i, and an associated response element, R_i, have thus occurred sequentially. Assume further that the sequence $S_i \rightarrow R_i$ is *unique*; i.e., that only S_i and R_i have ever been so connected. Clearly then, by comparing S_i with other stimuli S_j, S_k, to each of which several responses have become conditioned, its owner may exhibit S_i to be relatively high in *f*-value but quite low in *m*-value. The logical possibility of finding such unique *S–R* connections in *fact* is a prophylactic against using *m* and *f* as interchangeable terms.

From the analysis of meaning presented previously and from the Zipf-Thorndike findings, one may infer that verbal stimuli acquire *m*-value as some increasing function of their frequencies of occurrence (n). Symbolically, this relationship may be written

$$m = \Phi(n). \qquad [2]$$

If f were next shown to be some increasing function of n; i.e., if

$$f = \Psi(n), \qquad [3]$$

then it validly may be concluded that f is therefore some increasing function of m:

$$f = \Gamma(m). \qquad [4]$$

One may now proceed to test the deduction described in equation [4] by determining the empirical relationship between the meaning index (m) and judged frequency or familiarity (f). In Fig. 4 are plotted the results of a comparison of the *f*-scale with the *m*-scale. Familiarity (f) is shown as an increasing curvilinear function of the meaning index (m). The equation describing this relationship, determined by a least squares solution, is

$$f = 6.04(1 - 10^{-0.067m}) + 0.28. \qquad [5]$$

The index of correlation p, computed from the reduction line, has a value of 0.92.

It was noted in (10) that the response-frequency distributions for the stimuli on the *m*-scale were positively skewed for low values. In view of this fact, medians were computed in Fig. 4.

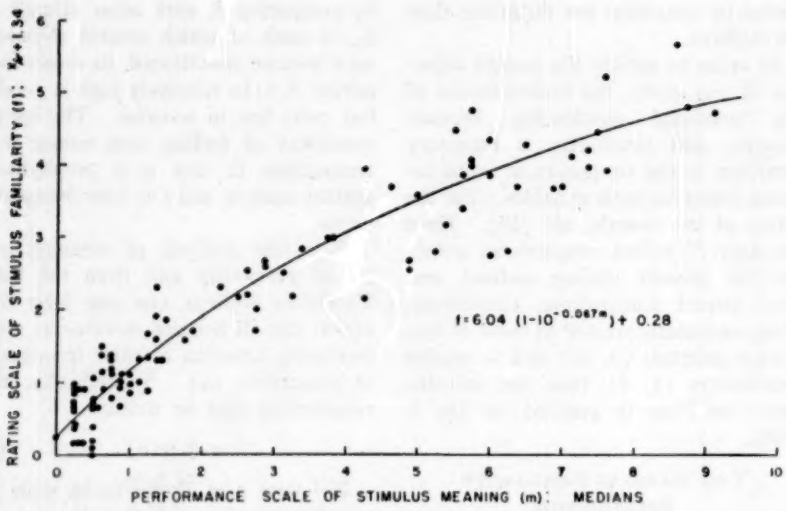

FIG. 4. Scattergram of the relationship between familiarity (f) and meaning (m). Ordinate values denote graded dichotomies scale values from ratings on Familiarity Schedule ($N = 200$). Abscissa values denote corresponding median frequencies of continued association to each of 96 dissyllables within 60 sec. ($N = 119$). The equation and fitted curve were derived from the method of least squares.

This measure of central tendency affords a more valid estimate of the zero point of the m-scale for the present purposes. It is clear that f is a negatively accelerated positive function of m. The Γ-function is neither one of identity nor of proportionality, a fact consistent with the preceding logical analysis.

The empirical law represented in equation [5] does not imply that *m determines f* in any sense of strict implication, such as that denoted by the assertion: "If and only if $m >$ zero, then $f >$ zero." It is only to say that f conveniently may be represented as a correlate of m. Obviously, there might be a third variable with which m and f are co-correlated; e.g., n, as suggested above. Hence, the hypothesis may be entertained that verbal stimuli acquire the attributes m and f as joint functions of frequency of stimulation.

The stimulus frequency hypotheses represented by equations [2] and [3] above are not regarded as verified, but as only tenable pending further tests. One attempt to falsify them might consist in experimentally establishing various m-values for a set of *new* verbal stimuli in a group of *Ss*, then having those *Ss* later rate the same stimuli for familiarity. This would provide a check on an hypothesis the evidence for which is at present only indirect; i.e., this analysis is concerned with *individual-difference* as distinguished from *experimental* variables. The suggested test would involve the "creation" of new stimulus attributes rather than assaying those already "built into" the *Ss* as a consequence of their prelaboratory experiences. The test might also be extended to nonverbal *S–R* situations, such as classical conditioned responses or perceptual-motor learning tasks.

In this context, the procedure of endowing stimuli with the properties of meaningfulness (m) or familiarity (f)

may constitute one unambiguous definition of Thorndikian "identifiability," which in turn may be related to such current notions as "pre-differentiated structure," "distinctiveness," "cue-value," and "recognizability." Noble (11) has shown, for example, that the variable *m* plays an important role in verbal learning.

It should be noted that the rationale governing the graphical representation of the data in Fig. 3 in the form $f = \Gamma(m)$ is that of the distinction Bergmann and Spence (2) have made between *strict behavioristic* terms and those which are merely *behavioristic*. This is further related to their distinction between *S–R* and *R–R* laws (cf. also 13). Adopting their symbolism, one writes the meaning-familiarity function $R_1 = \Gamma(R_2)$. The empirical relationship is expressed in this form for purely methodological reasons. It amounts to considering the regression of test scores on criterion scores. In other words, a distinction is made between ratings and performance. It should be emphasized that the *m*-scale provides fundamental measures (average frequencies) with an absolute zero point, while the *f*-scale yields derived measures (x/σ transformations) having an arbitrary origin. For any accurate quantitative purposes, therefore, the *m*-scale possesses certain advantages. It should also be recalled that an *m*-value is obtained by one simple, independent operation (mean or median) upon a single distribution of responses, while an *f*-value involves many complex, dependent operations upon (preferably) all the distributions of a set.

SUMMARY AND CONCLUSIONS

This paper has presented a theoretical-experimental analysis of the attribute of familiarity in verbal stimulus material. A word list of 96 dissyllables consisting of nouns and paralogs was presented to a sample of 200 U.S.A.F. recruits in order to establish a quantitative scale for this attribute. The results of this analysis were as follows:

1. Familiarity was formally defined as a stimulus attribute which is some increasing function of the frequency of occurrence of a given stimulus.

2. An index of familiarity (*f*) was operationally defined in terms of a five-point graphic rating schedule.

3. An equal-interval rating scale of *f*-values was developed which exhibited a range extending from 0.00 to 5.66. A test of internal consistency indicated an average error of 0.019 in reproducing the original data from the scale values.

4. Certain characteristics of the *f*-scale relevant to the theory of measurement were discussed.

5. The hypothesis was proposed that stimuli also acquire the attribute of meaning (*m*) as some monotonic function of frequency, and that, therefore, *f* should be positively related to *m*.

6. An attempt was made to determine the exact functional relationship between *m* and *f*. The equation was

$$f = 6.04(1 - 10^{-0.067m}) + 0.28.$$

7. The significance of these findings for research in verbal and in perceptual-motor learning was discussed.

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HOW ARE MOTIVES LEARNED?

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One of psychology's basic problems is to account for the variety, no less than the uniformity, of human wants. At one time the instinctivists seemed to hold the key; at another, the advocates of conditioning. When both of these doctrines in their early forms proved inadequate, students of behavior still faced the problem of bridging the gap between the bodily needs of the infant and the adult's desires to succeed or fail, to help or hurt, to be loved or hated.

Equally dissatisfied with the instincts of the hormic and psychoanalytic schools and with the conditioning of biological drives, Allport proposed the "functional autonomy of motives" as "a declaration of independence for the psychology of personality" (1, p. 207). But since he neglected to write a constitution for the newly liberated discipline, his principle did little to solve the problem. His most explicit statement was a paraphrase of Woodworth's original suggestion that in the course of activity *mechanisms* become *drives*; "activities and objects that earlier in the game were *means* to an end now become *ends* in themselves" (1, p. 195). How this transformation occurs was left untouched.

Another writer to grapple with the problem was Murphy (25). Explicitly rejecting functional autonomy, he broadened the base of unlearned drives and introduced the concept of *canalization* to represent the process by which they become focused on specific satisfiers. But to substitute a canalized drive for an autonomous motive is hardly explanatory. Both concepts belong in the category of "nominal theories" (32).

A third formulation that falls in the same class is Tolman's (33) drive-conversion diagram. When biological drives are frustrated, they are converted into social drives or techniques (e.g., self-assertive or collective) by the mechanism of learning and the dynamism of fixation. A technique is fixated to the extent that it persists in situations to which it is inappropriate. But to this extent it seems to become a drive, or at least equivalent to Allport's autonomous motive. If we only knew the conditions of fixation! With characteristic candor Tolman called his diagram "a scheme more for the asking of important questions than for the presenting of final answers" (35, p. 227).

A search for more precise conditions of motive learning calls for a distinction between learned drives and learned rewards. If a *drive* is learned it means that an originally neutral stimulus will instigate activity even when the primary drive conditions are removed. But if a *reward* is learned it means simply that activity otherwise instigated is *directed* toward an originally neutral stimulus. In the first case motivation is at least temporarily free of its primary source; in the second case it may or may not be. Whether reward learning implies some degree of drive learning as well is, in fact, the chief problem of this paper.

A second distinction empirically related to the first is between *appetites* and *aversions*, to borrow Tolman's terms. For our purpose an aversion may be defined as a motive started by the presence of a certain stimulus and ended by its removal. An appetite is a motive *ended* by commerce with a certain stimulus, but what starts it we cannot say without knowing more about the part played by learning. On the whole,

as we shall see, aversions lend themselves more directly to drive learning, while appetites seem more prone to produce and be produced by the learning of rewards. This difference is, I believe, too often glossed over. Tolman, for example, in his latest discussion of the problem, described *positive* and *negative cathexes* as "attachments of specific types of final positive goal-object, or of final negative 'disturbance-object' to basic drives" (34, p. 144). (The basic drives are presumably appetites and aversions, respectively.) Although Tolman treated them as strictly analogous, on closer inspection the parallel breaks down. For one thing, response to the positively cathected goal-object comes *after* the drive is in force, while the negatively cathected disturbance-object comes *before* the drive and serves to arouse it. For another, positive cathexes are reinforced, for Tolman, by need-reduction, but negative ones are reinforced by "pain or some other type of noxious physiological state" (34, p. 147). Thus it is not at all clear that "cathexis learning" is a unitary construct either in method or result.

Mowrer (22) recognized this potential split without pursuing its implications. In asserting that drives and other visceral responses are learned by contiguity and skeletal ones by the law of effect, he emerged, as he put it, with one principle for the making of problems and another for solving them. Since his dichotomy grew out of experiments involving fear and its reduction, aversions fit readily into his scheme. But appetites are left in an ambiguous position. Is goal anticipation problem making or problem solving? Drive increasing or drive reducing? Conditioned or "reinforced"? In discussing the Pavlovian experiment Mowrer suggested that the salivary CR be considered "an indication, perhaps a veritable part of the mechanism, of the secondary motivation known as 'appetite' " (24, p. 257). But even as such it may mean merely that the dog is hungry and expects food, not that he has acquired a desire for it whether hungry or not.

Hull expresses similar uncertainty with respect to the systematic status of incentives. On the one hand he developed the functions of the fractional antedating goal reaction and its proprioceptive consequent in purposive behavior (13, 14, 15). On the other hand he made room for secondary motivation through the reinforcement of connections between neutral stimuli and response-produced drive stimuli. In his latest publication he writes: ". . . the understanding of . . . secondary motivation is rather obscured by the fact that its relationship to incentive motivation . . . and to the associated mechanisms of anticipatory goal reactions . . . has not yet been clarified" (16, p. 24).

A major attempt to apply a drive-learning formula to appetites was made by Anderson (2). He proposed that the neural drive mechanism, at first aroused by internal conditions, becomes externalized, i.e., activated by external situations in which it has been repeatedly satisfied. Anderson's theory is important both because of its experimental fertility (3, 4, 5) and because it may even be true. As it stands, however, it illustrates some of the difficulties we must face. What has the *satisfaction* of a drive to do with its externalization? How is a drive, externalized to a goal object, aroused in its absence? How does an externalized drive cause persistent behavior in a satiated organism but not consumption of the goal object?

Miller, who leads the experimental attack on the learning of motives, has recently summarized research in this area (21). His review bears out our impression of an asymmetrical development. Writing on "learnable drives and rewards," Miller devoted 18 pages to the

acquiring and eliminating of fears. Of eight pages on food hunger, six dealt with the learning of rewards, including a table of 21 studies, the other two with the learning of drive. Of the experiments cited in these two pages, only three or four could be considered at all definitive. If fear and hunger are typical we must conclude that aversions yield plenty of evidence for drive learning and that appetites show plenty of reward learning, but that evidence for the learning of appetitive drives is scanty.

One reason for this discrepancy may lie in a difficulty of experimental technique. To demonstrate secondary motivation, the influence of the primary drive must be ruled out. In aversions this is easy; one need only turn off the shock. But how can the effect of hunger be eliminated? The most obvious way is to feed the animal to the point of refusal. To reach this point, however, it may not be enough to reduce the need for food to zero, since we must counteract not only a metabolic deficit but also any acquired drive to eat. But this means removing a part of the very process we seek, the remainder of which we must try to measure by some other response. This argument is not entirely circular. Most experiments on the strength of food-getting responses as a function of hunger (12, 17, 27) agree that the curve shows a break at about two hours of deprivation, rising steeply below that point and only gradually above it. These data suggest that satiation introduces a factor (full stomach?) that inhibits activity associated with food. Until other means of control are devised, learned appetitive drives may well continue to elude us.

A second reason why aversions may be easier to condition than homeostatic drives was suggested by Miller (21, p. 463) and has to do with their relative abruptness of onset. To be conditioned, a response must be evoked in the presence of the CS. Hull states explicitly that for secondary motivation to develop, neutral stimuli must be "associated with the *evocation* of a primary or secondary drive" (italics mine) (16, p. 25). Escape drives can be aroused by noxious agents applied suddenly in conjunction with neutral stimuli; hunger or thirst, on the other hand, develops slowly. The same reason may explain why appetites show readier conditioning of consummatory than of drive-producing responses.

These and possibly other hypotheses must be explored before the data can be finally evaluated. Meanwhile it may help to face squarely the issue before us. Experiments strongly imply that the chief sources of motivation are a few bodily needs, a variety of ways of satisfying them, and a large number of fears. Common experience, on the other hand, suggests that the bulk of our activity is directed toward things we want rather than away from things we don't want, and that many of the things we want have little to do with bodily needs. How can these conflicting lines of evidence be reconciled?

Several ways are open. An extreme behaviorist, for example, will limit his materials to experimental observations and closely related constructs and will stretch them as far as he can. The theory he fashions may run something like this: The only unlearned drives are bodily discomforts and the only learned drives are fears. The apparent motive force of positive desires is illusory; a cigarette craving is anxiety, sociability is a fear of solitude. Goals are not really motives at all, but habits acquired by drive reduction, activated by drives, and capable of providing secondary reinforcement for other habits. The few cases in which subgoals have been found to reinforce habits in satiated animals may be explained by Mowrer's concept of *drive-fear* (23). A well-fed, warmly

clad wage earner seeking a raise is driven by hunger-fear or cold-fear. At the other extreme a "personalistic" psychologist may describe the growth of human aspirations with little regard for laboratory results, on the assumption that these are largely irrelevant to personality dynamics.

The theory in the next section is presented as an attempt to account for the experimental evidence on learning drives and rewards and also, it is hoped, as a step toward the identification of other factors for which experimental evidence is not yet available.

THEORY OF MOTIVE-LEARNING

In essence I suggest that acquired drives fall into two classes, secondary and tertiary, depending on how they are acquired. Secondary drives result from the conditioning of primary drive components to external stimuli. Tertiary drives result from the blocking of learned responses, especially responses by which primary or secondary drives have been reduced. Which kind of drive is acquired depends on whether circumstances favor the conditioning of drives or the learning of goal responses. There is no *necessary* connection between this classification and that of primary drives into aversions and appetites. As Miller has pointed out, however, the onset of aversions is typically sudden; that of appetites, gradual. It seems reasonable to suppose that in order to be learned a response must involve a somewhat abrupt change of excitation. We should, therefore, expect aversions to yield secondary drives supplemented by tertiary, while appetites, *unless their onset is sudden*, should lead almost exclusively to tertiary drives.

The most convincing evidence of acquired motivation is the learning of a new response in the absence of primary drive. Our theory should therefore enable us to deduce this finding both where conditions favor drive-learning, as in aversions, and where they favor reward learning, as in appetites. To be of value the theory should also "explain" other pieces of evidence, generate additional hypotheses to test, and permit expansion to include a wider range of behavioral phenomena. With these criteria in mind I submit the following definitions and postulates.

Definitions

D1. *Surrogate response* (rs). A central neural process by means of which a stimulus (S) evokes a response (R).[1] Strength of R, as measured by amplitude, latency, relative frequency, or extinction rate, is a function of intensity of rs. Under special conditions (to be stated in other definitions and postulates) rs can function independently of its primary S or R.

D2. *Conditioning*. A rs_1 is said to be conditioned to rs_2 when rs_1 acquires a *functional connection* with rs_2. Such a connection is called *adequate* in so far as rs_1 is actually followed by rs_2; it is called *potential* in so far as arousal of rs_2 must await the aid of other S's or rs's.

D3. *Primary drive stimulus* (S_D). A S producing or produced by a homeostatic imbalance.

D4. *Drive* (rs_D). A rs first aroused by S_D or by postulate 4 and able to strengthen the rs's to a hierarchy of R's as an increasing function of its own intensity.

D5. *Primary goal stimulus* (S_G). A S producing or produced by a homeostatic balance (G) with respect to a particular need.

D6. *Goal surrogate* (rs_G). A rs first aroused by S_G or by postulate 4 and varying in intensity as a negative linear function of the intensity of rs_D.

[1] The term *stimulus* and *response* are used for convenience to refer to patterns of receptor and effector activity of varying complexity.

D7. *Subgoal response* (R_g). A R closely and consistently followed by an abrupt increase in intensity of rs_G.

Postulates and Corollaries

P1. *Association.* If rs_1 is active when rs_2 is aroused, rs_1 becomes conditioned to rs_2. Strength of conditioning is a positive function of the number of concurrences and a negative function of the time interval between $S_1 - R_1$ and $S_2 - R_2$.

C1. *Secondary drive* (rs_{D_2}). A rs accompanied by an abrupt increase in intensity of rs_D becomes conditioned to it and its S becomes a *secondary drive stimulus* (Srs_D).

C2. *Secondary reward.* A rs accompanied by an abrupt increase in intensity of rs_G becomes conditioned to it and its S becomes a *secondary goal stimulus* (Srs_G).

P2. *Activation.* The strength with which rs_2 is activated by rs_1, as a result of a potential or adequate connection, is a positive function of the (*a*) strength of connection, (*b*) present intensity of rs_1, (*c*) intensity of rs_2 during conditioning.

P3. *Facilitation.* An increase in intensity of rs_G strengthens the rs of any R in progress as a positive function of the amount and rate of increase.

P4. *Tertiary drive* (rs_{D_3}). If rs_1 has a strong potential connection with rs_2, arousal of rs_1 produces a drive (rs_{D_3}) varying in intensity as a positive function of strength of activation of rs_2. Arousal of rs_2 reduces the drive, the amount of reduction varying positively with the intensity of arousal.

P5. *Summation.* If two rs's conditioned to the same rs_3 are simultaneously active, they arouse rs_3 with a total intensity which is greater than that evoked by either one alone.

A word must be said about the two pivotal concepts, *drive* and *goal surrogate*. Although given the status of rs and thus subsumed under P1, they have certain special properties. The rs_D is a central excitatory state, while rs_G is a state of diminished excitation negatively related to it. Although it may ultimately prove unnecessary to retain both constructs, at present it seems to me to simplify the theory. For one thing, it avoids the awkward notion of conditioning the reduction or termination of a process. A rather specific relationship is envisaged here: i.e., to the extent that two rs_D's are discriminable the corresponding rs_G's are likewise distinct; to that extent rs_{D_1} and rs_{G_2} vary independently of each other. The rs_G produced by eating, for example, reduces only slightly if at all the rs_D of sex and is but little reduced thereby. Hunger and thirst, on the other hand, are so closely related that it would not be safe to make the same assumption regarding them (8, 36). Some drives, too, may have proprioceptive components in common and, at high intensities, visceral ones as well. Such overlapping complicates the problem of isolating learned drives. In so far as it can be made, the assumption of independence is important for us because of the following consequence: *if a given* $rs_D = 0$, *the strength of its* rs_G *cannot be further increased.*

Despite certain changes the postulates have a somewhat parasitic relation to Hull's system and do not pretend to be self-sufficient. My chief departures from Hull are two: (*a*) Drive reduction is believed to strengthen, not the conditioning of R's, but the R's themselves; (*b*) the concept of *surrogate response* replaces his response-produced stimulus and is given a focal position. P1, for example, may be recognized as a modified version of Hull's postulates 4 (habit formation) and 8 (delay in reinforcement) (16), restated in terms of rs's and without reference to reinforcement. P2 is a partial restatement of his postulate 9 (constitution of reaction potential).

"Strength of conditioning" and "strength of connection" are synonymous and are equivalent to Hull's *habit strength*, while "intensity of rs" is the counterpart of his *reaction potential*. The function of drive reduction in the present theory is embodied in P3. Note that it affects activity in progress, not strength of connections, another way of saying that "reinforcement" is here thought of as a principle of performance rather than of learning.[2]

P4 attempts to state the basic mechanism whereby a goal acquires motivating power. A similar idea has been formulated by Whiting in an unpublished manuscript cited by Miller (21); P4 is, therefore, not original except in its present form. Emphasis should be placed on the concept of *potential* as distinguished from *adequate* connection; i.e., a habit becomes excitatory when it is started but not finished.[3] As it stands, the postulate is exceedingly primitive. It needs first of all a statement of the conditions that result in potential connections. One example is a R that cannot be completed without an external S; one cannot eat without food or press a bar that isn't there. Another is a time interval between S_1 and S_2; presumably the shorter the time the more adequate the connection, a relation that may be reduced, as Spence (31) has suggested, to distance on a generalization gradient. A second problem concerns the quantitative relation between activation of rs_2 and strength of rs_{D_2}. We are familiar with the heightening of tension as we approach a goal, yet we should expect rs_{D_2} to be reduced by the more adequate arousal of rs_2. A solution might be to break down purposive behavior into a series of subgoals; as each one is attained, it reduces its own rs_{D_2} but adds potential leading to a later stage. Such possibilities must be left for future development.

[2] In view of the inverse relation between rs_D and rs_θ it is tempting to suggest a counterpart of P3; viz., an increase in intensity of rs_D weakens the rs of any R in progress. Since this hypothesis is not needed here, its possibilities will not be developed.

[3] Compare with Allport's suggestion that it is a habit-in-the-making that becomes autonomous (1, p. 204 f.).

Deductions

1. *Response selection under learned drive.* So far the above propositions, with the help of a few others, have led to the more or less formal deduction of nine theorems dealing with one or another aspect of acquired drives. Only those directly concerned with R selection under rs_{D_2} and rs_{D_3} will be reproduced here; others will be merely indicated along with the supporting evidence.

First we need a general derivation of the phenomena usually attributed to selective learning. Theorem 1 is an attempt to meet this need.

T1. *Response selection. Given:* On one or more occasions an organism (O) under drive (rs_D) in the presence of S makes a certain subgoal response (R_g).

To prove: On recurrence of rs_D and S the probability of R_g will be greater.

Proof: 1. Let R_x be any other R *not* R_g made in the presence of rs_D and S. (For simplicity let R_x and R_g be equivalent in other respects; e.g., availability, effort, previous conditioning.)

2. R_g, but not R_x, is followed by rs_G (D7).

3. rs_g is conditioned to rs_G more strongly than is rs_x (P1, C2).

4. S, by way of rs, is conditioned to both rs_x and rs_g, as is also rs_D (P1).

5. The next time S and rs_D arouse rs_g they will induce a greater intensity of rs_G than when they arouse rs_x (Step 3, P2).

6. In turn, rs_G will strengthen rs_g more than rs_x (P3).

7. The frequency of R_g relative to

R_x—i.e., its probability—will be increased (D1). Q.E.D.

To go from T1 to the special case of R selection under rs_{D_2} is only a step, as shown in theorem 2:

T2. *R selection under rs_{D_2}. Given:* (1) A neutral S is accompanied one or more times by an abrupt increase of rs_D. (2) S is later presented alone and removed when O makes a certain R.

To prove: On recurrence of S the probability of R will be greater.

Proof: 1. After one or more conjunctions with a sudden increment of rs_D, S alone arouses rs_D (C1).

2. Removal of S results in an abrupt increase of rs_G (D6).

3. R thus becomes R_g (D7).

4. The conditions of T1 are thus fulfilled and R will occur with increased probability in the presence of S.

Q.E.D.

Brown and Jacobs (7) provided the experimental model for this theorem. After repeatedly giving rats a neutral S paired with shock, they presented S alone and removed it when the animal jumped a barrier. They found a progressive decrease in latency of jumping, a result that could just as easily be deduced as an increase in probability (the one, in fact, implies the other).

Closely resembling the Brown-Jacobs experiment was its forerunner, Miller's (20) classical demonstration of fear learning. In that study, however, on training trials a running R regularly led to escape from shock. When this R_g was prevented by closing the door, it could have generated rs_{D_2} as the motive for learning a door-opening R. Indeed, it was to eliminate this alternative that Brown and Jacobs made their changes. Since Miller's experiment, for all its value, could be explained by either rs_{D_2} or rs_{D_3}, it was crucial to neither.

Our next objective, therefore, is to derive a more rigorous test of rs_{D_3}. T1 provides for the selection of R_g under drive. But to free rs_{D_3} from other drive effects we must be able to predict that when S is later presented without either primary drive (rs_{D_1}) or rs_{D_2}, it will still tend strongly to arouse R_g. Note that if we assume a minimum of drive interaction, and $rs_D = 0$, rs_G cannot be increased and therefore cannot facilitate rs_g. With the help of P2, however, the deduction is readily made, as shown by theorem 3:

T3. *Response persistence. Given:* the same conditions as in T1, save that R_g must occur on *more than one occasion* during training.[4]

To prove: On recurrence of S in the absence of rs_D, the probability of R_g relative to other R's will be greater.

Proof: 1. Again let R_x be any other equivalent R not R_g.

2. After one trial under rs_D, due to facilitation by rs_G S is subsequently conditioned to a greater intensity of rs_g than of rs_x (Steps 2 to 5, T1).

3. On later recurrence without rs_D, S will still arouse rs_g more intensely than rs_x. (By the last provision of P2 the intensity of rs aroused is a positive function of the intensity of rs conditioned.)

4. S will evoke R_g more readily than R_x (D1). Q.E.D.

T3 is not without evidence. Teel and Webb (32) trained rats to food on one side of a T after varying amounts of deprivation. Every day after two deprived trials to each side they gave a pair of satiated trials. The percentages of correct choices under deprivation and satiation were practically the same throughout training. Seward and Handlon (28) gave thirsty rats an equal number of forced trials to water on one side

[4] The reason for this proviso will become clear in step 2 of the proof. Incidentally, since a reinforcement learning theory does not make this requirement, we have here a possible technique for putting the two hypotheses to a crucial test.

of a choice point and no water on the other. When satiated for water, the rats still showed a significant shift of preference to the water side.

Consider another implication of the theorem. Let us assume that rs_G can facilitate only under rs_D but that its effect persists when rs_D is removed. Let us further assume that two R's to S are both followed by Srs_G, but one (R_g) is made under rs_D and the other (R_x) is not. It follows that when S is presented without rs_D, R_g will be preferred to R_x. Seward, Levy, and Handlon (30) tested this implication. They forced rats equally often to water on both sides of a choice point, but thirsty to one side and satiated to the other. Tested under satiation the rats shifted their preference to the thirsty side.

We are now ready for an attempt to use R selection as a criterion of rs_{D_3}.

T4. *R selection under* rs_{D_3}. *Given:* (1) An O under rs_D in the presence of S_1 makes more than once a certain R_g that cannot be completed without S_g. (2) S_1 is later presented one or more times without rs_D, and with S_g presented only when O makes a certain R_c.

To prove: On recurrence of S_1 the probability of R_c will be greater.

Proof: 1. O comes to the test trials with a relatively strong tendency for rs_1 to activate rs_g (T3).

2. Withholding S_g creates the conditions for rs_{D_3} (D2, P4).

3. O makes a number of R's (D4). Let R_c be one of these and let R_x be any other R not followed by S_g.

4. The following connections are formed: $rs_1 - rs_x$, $rs_1 - rs_c$, and $rs_c - rs_g$ (D1, P1).

5. On subsequent trials, when rs_1 arouses rs_c, rs_g is evoked with greater intensity than when rs_1 arouses rs_x (P5).

6. But this added intensity reduces rs_{D_3}, thus increasing rs_G (P4, D6).

7. The conditions of R selection are thus provided, resulting in a greater probability of R_c than R_x (T1).

Q.E.D.

As already said, evidence for rs_{D_3}, free of rs_{D_2}, is most likely to be found where rs_{D_1} is a slow-rhythm appetite. But the experimental demonstration of acquired drive based on appetites, possibly due to the satiation effect already mentioned, has proved difficult. In a study so far only briefly reported Miller (19) trained two groups of rats to run from a white to a black box, one to escape shock and the other to allay hunger. He then compared their performances without rs_{D_1}. Not only did the shocked rats maintain their R_g longer than the fed ones but they learned a new R, whereas the other group did not. Under the usual experimental conditions rs_{D_3} seems definitely weaker than rs_{D_2}. Miller's report contained one bit of evidence for rs_{D_3}. He trained several groups of rats to run through a door to food, then tested the R_g under satiation. One group that had been blocked by the door for varying delays during training was the slowest to extinguish. The present theory would predict this result on the ground that blocking the door would generate rs_{D_3}, which would be perpetuated on test trials through the conditioning of a stronger rs_g.

Evidence from maze studies is more favorable. Some years ago Anderson (4) found indications that rats trained hungry and rewarded in one maze did better in a new maze, even though satiated, than a control group previously trained under less favorable drive-incentive conditions. He interpreted his results in terms of drive externalization. If his promising data were confirmed, I should prefer to say that training conditioned strong potential rs_gs to maze S's and that these rs_g's provided rs_{D_3} for the selection of R's leading to their completion. To predict Anderson's findings, T4 would have to be adapted only to

the extent of providing for stimulus generalization from one maze to another.

More recently MacCorquodale and Meehl (18) put food and water in the same endbox of a T. Rats satiated for both objects nevertheless chose that side significantly more than the empty one. In an unpublished study Myers (26) confirmed this result impressively. Putting a rat as a social incentive on each side of a T, but food on only one, he gave satiated rats two trials a day, one free and one forced opposite. In 12 days they increased their food-side choices progressively and significantly. When the food was then shifted to the other side for 26 days, they gradually reversed their preference, 21 out of 26 rats reaching a criterion of 8/10 "correct."

These two studies do not precisely fit the paradigm of T4 in that S_1, the choice point, was not combined with S_g, the incentive, during an initial training period under rs_{D_1}. "Training" presumably took place in *O*'s feeding history, during which the rs_g's involved in seeing and smelling food became strongly conditioned to—and thus facilitated by—the rs_G produced by eating. The results suggest that if S_1 is conditioned to a rs_g that is already strong enough, it will produce rs_{D_2} without further help from primary drive or reward.

Granting that food and water can bring about new learning in the absence of the original drives, we may ask if rs_{D_2} learning is confined to such "canalizers" or if it can be mediated by other objects not so intimately related to drive reduction? Anderson's study suggested the latter possibility. Seward and Levy (29) tested the hypothesis that rats rewarded in one compartment but not in another would show a preference, even when satiated, for the compartment that had previously contained the reward. They first trained thirsty rats in a runway to two different boxes, one containing water and the other none. (In contrast with the two studies just discussed, it was here found necessary to associate runway *S*s with endbox *S*s under drive-reward conditions in order to get positive results.) Later the rats were satiated and run in a T with one box on either side. Test trials gave clear and consistent evidence of the effect of training on choices, with highly significant differences in favor of the positive box. Moreover the presence or absence of food and water in the boxes during tests was apparently without effect. Its importance during *training*, however, was further brought out by interpolated runway trials under thirst. When water was removed from *both* boxes on these trials, the difference between them in satiated test choices dwindled rapidly. Apparently a need-reducing R_g that has become the basis of rs_{D_2} may lose that function if, when the original need is present, R_g no longer satisfies it.

2. Other evidence. Various signs of acquired drive in the experimental literature lend themselves to interpretation in terms of the present theory, but here they can be only briefly mentioned. (*a*) *Goal-instigated activity* was strikingly demonstrated by Geier and Tolman (10, 11). They devised a method of measuring *frustration-tension*, a construct that seems identical with tertiary drive. (*b*) *Response persistence* after rs_{D_1} is removed does not necessarily imply an acquired drive since it may be due to ${}_sH_R$ combined with an irrelevant *D*. But Farber (9) showed that *R* was abandoned more quickly when rs_{D_2} had been extinguished than when it had not. And Brogden (6) provided parallel evidence for rs_{D_2} when he retarded extinction of a flexion *R* in satiated dogs by "rewarding" it with a pellet.

UNSOLVED PROBLEMS

In the last section a theory of acquired drives was presented and ex-

amined in the light of experimental evidence. As anticipated, all the evidence for rs_{D_2} came from experiments using shock, while support for the concept of rs_{D_3} was drawn exclusively from experiments using hunger or thirst. This dichotomy arose as a natural result of the way the two concepts originated: the theory of secondary drive was based primarily on situations involving fear; that of tertiary drive grew out of studies on appetites. The question remains whether this is a distorted view or whether it represents a true distinction between appetites and aversions. How much of what we have called rs_{D_3} is actually "externalized" hunger? How much of fear is a desire for safety?

A way to find out is suggested by the experiments of Miller (20) and of Brown and Jacobs (7) already cited. The first of these studies provided conditions for acquiring both rs_{D_2} and rs_{D_3}; the second, for rs_{D_2} alone. If two such experiments were performed under strictly equated conditions the relative "weights" of the two sources of drive might be determined. And if a similar pair of experiments was carried out substituting hunger for shock, a direct comparison could be made in this respect between a typical aversion and a typical appetite.

Supposing that appetites and aversions do differ as suggested, how are we to explain it? Is Miller correct in supposing that relative abruptness of onset is the distinguishing feature? If so, a suddenly induced hunger should give rise to rs_{D_2}, while a gradually developing aversion should not; on the other hand, such an aversion, if suddenly relieved, should yield rs_{D_3}. All we need is a way of reversing the usual time relations.

Even if we accept the idea that an appetite generates rs_{D_3}'s we must still face the question: why are they so much more elusive than fears? There are several possibilities to be explored. One factor already mentioned is the counteracting of acquired motives by acute satiation of the primary need. A method is required for isolating goal-instigated behavior without stuffing the animal. A second hypothesis is that goal and subgoal responses must somehow be made stronger before their incipient arousal can act as a drive. Perhaps this could be done by increasing the amount of training, or strength of rs_{D_1}, or the number of different needs satisfied, or the effort required to make R_g. Thirdly, it is possible that rs_{D_3} is a fairly high-level cerebral achievement; as such it would probably be easier to develop in animals with bigger brains than rats.

SUMMARY

This paper starts with a dilemma. On the one hand human activities seem to be largely instigated by goals that have little to do with bodily needs. It has been suggested that primary drives are conditioned to external objects, which come to arouse them independently of tissue disturbance. On the other hand experiments with animals have shown that, although fear is easily learned, the appetitive drives are not. At the same time goal-associated responses are readily "cathected" in connection with these drives. How can the facts of life and the facts of the laboratory be reconciled?

A theory is proposed, the gist of which is that there are two ways by which motives can be learned. Secondary drives are conditioned directly to neutral stimuli; tertiary, or goal-instigated, drives are touched off by the obstruction of a learned response. Owing, perhaps, to a difference in characteristic rates of onset, aversions give both kinds of drives but appetites give mainly the tertiary type. To make possible a more precise integration of theory and experiment a set

of definitions and postulates is offered and several illustrative theorems are derived. These are discussed in relation to available evidence.

But the initial dilemma remains to challenge the ingenuity of future experimenters.

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LEARNING THEORY AND "ABNORMAL FIXATIONS"

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In a recent article (5), Maier and Ellen, supplementing Maier's original thesis (4), have attempted to show that the persistent behavior induced in animals by certain conditions of stress cannot be understood in terms of learning concepts and must therefore be regarded as having a nonlearning origin which Maier has labeled "abnormal fixation" and which he believes to be a result of continued frustration often exacerbated by punishment. (The implication is that this behavior has as a basis a special kind of organic change qualitatively different from the neural modifications presumed to subserve learning (18).)

Maier and Ellen present their case in two parts. In the first they argue that certain features of the experimental results obtained by other workers are not satisfactorily covered by Mowrer's anxiety-reduction theory (13, 14); and in the second, that certain of the experimental findings in Maier's laboratories are inexplicable in terms of learning theory. In both cases they hold out frustration theory as the satisfactory alternative. The aim of the present paper is to show that learning theory is quite able to explain all the experimental facts, provided that it is remembered that there are reducible drives other than learned anxiety.

Comments on Some Data Obtained by Anxiety-Reduction Theorists

Maier and Ellen agree that in experiments such as those of May (10), Miller (11), and Mowrer (14) there is a learning of avoidance responses on a basis of anxiety reduction, but dispute the adequacy of this mechanism to explain all aspects of the experiments reported by Farber (2), Mowrer (15), and Mowrer and Viek (16).

Their quarrel with Farber rests entirely on the allegation that he does not account for the outstandingly great difficulty some of his rats had in extinguishing their anxiety-based responses. Yet Farber went to considerable pains to show that these animals came from strains that were more emotional generally. This seems a reasonable explanation, for higher emotionality would imply that stronger learnable anxiety responses would be evoked by the shock, and these would also be more strongly reinforced because higher measures of anxiety drive would be reduced. The effect of this would be that during the extinction series extinction would be counteracted by the reduction of unusually great learned anxiety drive.

In Mowrer's experiment (15) a rat was conditioned to run from the left end of a 4-foot alley to a safe compartment at the right end by consistently electrifying the floor of the alley 10 seconds after the introduction of the animal. When the habit of running within the 10 seconds' grace was well established, the *right half* of the floor was *permanently* charged. It was then soon found that the animal placed at the left end ran even more promptly to the safe compartment, and this behavior persisted for hundreds of trials.

Maier and Ellen reject Mowrer's "two-factor learning theory" explanation of this experiment, rightly, it seems, for one of Mowrer's main arguments in support of his theory has recently been shown fallacious by Miller (12) on the basis of an experiment by Sullivan (17). But this does not mean that we must fall

TABLE I

NUMBER OF RESPONSE INHIBITIONS OBTAINED IN SHOCK-CONTROL GROUP AND SHOCK-UNCONTROLLED GROUP

Successive days	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	Total
Shock-control group	0	0	1	0	2	2	2	3	1	1	1	1	1	1	0	16
Shock-uncontrolled group	0	1	4	5	7	6	7	4	7	7	8	8	8	8	7	85

Cited from O. H. Mowrer and P. Viek (16, p. 396).

back on frustration theory, because, in fact, the results of the experiment are quite understandable in terms of established principles of learning, as follows.

During the early part of the experiment, emotional and running responses are reinforced to alley cues by reduction of the shock-induced drive when the animal reaches the safety compartment. Whenever the animal makes a run without being shocked, this constellation of responses is reinforced by anxiety-drive reduction. Now, when the right half of the alley is permanently charged, the ongoing responses of the running rat receive stronger reinforcement to the cues of that part of the alley, because when the safe compartment is reached, there is also reduction of the added measure of drive provided by the shock. Since these cues consist of wall and floor stimuli indistinguishable from those in the left half of the alley, it is not surprising that the animal's response of running when placed in the left half is reinforced and kept at a maximum by consistent shocks in the right half. This interpretation might be decisively tested by repeating the experiment with one half of the alley black and the other half white.

In Mowrer and Viek's experiment, (16) performed on two groups of 10 rats, the hungry rat was placed on the electrifiable floor of a rectangular cage, and offered food on a stick for 10 seconds. Whether the animal ate or not, shock was applied 10 seconds later. In the case of one group of ten rats, jumping into the air resulted in the experimenter switching off the shock (shock-controllable group). Each animal in this group had an experimental "twin" to which the shock was applied for the same length of time as it had taken its counterpart to jump into the air (shock-uncontrollable group). One trial a day was given to each animal. The number of animals in each group whose eating response during the 10 seconds was inhibited was charted each day with results shown in Table 1.

Mowrer and Viek try to explain the much smaller development of eating inhibitions in the shock-controllable group on the hypothesis that these rats have learned a "symbol for leaping" that somewhat alleviates their fear both before and during the shock. "By thus 'thinking' how the shock can be gotten rid of, the rat can presumably lessen the attendant fear of shock" (16, p. 198).

It is evident that *something* must be happening to check the development of fear responses to the stimulus situation that precedes the shock, but there is good reason for denying that the relevant occurrence is the evocation by this stimulus situation of a "thought" of the drive-reducing jumping response. The objection is that although states of drive can, through learning, become evocable by new cues, there is no evidence that this ever happens to drive *reductions*. *Thinking* how lunch may be obtained in

no wise appeases one's hunger. Cues along the learned pathway to the reduction of a drive do not reduce that drive. (See the writer's criticism (21) of Hull's use of "fractional goal responses" (3, p. 100).)

However, learning theory *can* explain how the reinforcement of the jumping response in the shock-controllable group could have brought about a diminishing level of anxiety in these animals. When an animal is subjected to continuous electric shock for the first time in a given situation, autonomic responses (anxiety) and numerous musculo-skeletal responses are evoked. A similar variety of musculo-skeletal responses will accompany the autonomic responses at every repetition of the electric shock unless there is one musculo-skeletal response that is repeatedly followed by cessation of the shock. If there is a response thus repeatedly reinforced, it will, of course, become increasingly dominant over all the other "competing" response tendencies. This implies a gradual weakening of the latter, probably by conditioned inhibition based on reciprocal inhibition (20). It is not unreasonable to suppose that *the weakening of "competing" response tendencies extends also to the autonomic responses.* Thus, in Mowrer and Viek's shock-controllable group, the repeatedly reinforced jumping response might have been expected to inhibit reciprocally the anxiety responses to some extent, thus slowly weakening their habit strength even though reduction of the shock-induced drive state would tend to reinforce both the jumping and the anxiety responses.

It appears from both experimental (19, 20) and clinical (22) observations that for a response to be weakened on the basis of reciprocal inhibition *the simultaneous competing response must be the dominant of the two.* This implies that the latter must be more "massive," i.e., subserved by a greater measure of central neural excitation (18). The jumping response reinforced by Mowrer and Viek was obviously rather massive; and it is conjectured that if they had chosen to reinforce, say, head turning to the left instead of jumping, the shock-controllable group would have developed much more anxiety than it did. Tending to support this conjecture is the fact that in the typical Cornell technique for producing experimental neuroses (e.g., 1) a very localized musculo-skeletal conditioned response comes to be increasingly dominated by autonomic anxiety responses (by a process discussed elsewhere (19)).

COMMENTS ON "ABNORMAL FIXATION" EXPERIMENTS

In Maier's laboratories rats were trained to jump to differential cards in a Lashley jumping apparatus. Jumping to the unlatched "correct" card was rewarded with food, and to the unlatched "incorrect" card punished by a bump on the nose and a fall on to the net below. If, after the animal had learned a jumping habit, the cards began to be latched in random fashion, the animal would soon refuse to jump. It would then be forced to do so, usually by blowing a blast of air on to it. The jumping so compelled followed a pattern that would persist unchanged for hundreds of trials. Maier (4) called this "abnormal behavior fixation."

Maier and Ellen explore whether or not a learning-theory hypothesis will explain this phenomenon. They suppose that the air blast could result in anxiety becoming conditioned to the experimental situation. Then, jumping in a given way becomes persistent because, despite other changes in the situation, it continues to reduce the conditioned anxiety. Viewed in this way, ". . . an abnormal fixation would be considered as a learned adjustive mechanism." Maier and Ellen state that "on the surface"

this hypothesis seems satisfactory, and then proceed to point out a number of experimental facts that it fails to explain.

There is little doubt that the experimental facts they give do invalidate the above learning-theory hypothesis. But, as will be seen below, the same facts are quite in keeping with the differently formulated hypothesis that follows.

Each time a jump is forced by the air blast, it is reinforced by reduction of the air-blast-induced drive. This is *a primary drive*, and its reduction is clearly overwhelmingly responsible for the reinforcement; for the platform situation is at no stage conditioned to a sufficient level of secondary drive to be able to impel jumping in the absence of the air blast. When jumping in a particular way has thus been repeatedly reinforced, it becomes firmly established as the habitual response to the air-blast stimulus, and the more firmly it is established the weaker does the competing alternative response tendency become.

Armed with this interpretation we are in a position to deal with all of Maier and Ellen's objections, and we shall do so in the order followed by them.

1. If, for a fixated animal, one card (positive) always leads to food and the other (negative) to nose bumping, the strength of air blast needed to cause jumping gradually comes to be less when the positive card is on the fixated side than when the negative card is there (7). Maier and Ellen say that as this shows that the animal has learned to discriminate between the two cards, it should now jump to the positive card even when it is on the nonfixated side, and as it does not do so the fixation cannot be based on learning. But they overlook the fact that *the stimulus to jumping is the air blast and not the card.* The air blast inevitably evokes the one response that has been reinforced to it. Conditioned response tendencies to other stimuli in the situation can only be expected to modify quantitatively the conditioned jumping response.

2. Maier and Klee (8) found that if a jumping response to one side has become fixated, and an attempt is made to alter this behavior by latching the card on that side so that the rat is punished by bumping its nose, fewer changes occur if the animal is punished at every jump than if it is punished on only 50 per cent of trials, receiving food reward on the platform beyond the unlatched card on the other 50 per cent. Maier and Ellen correctly point out that this result cannot be explained in terms of the anxiety drive associated with the punishment. But it can be explained in terms of the drive due to the air blast. As mentioned above, repeated punishment increases the strength of air blast required to produce a jump towards a given card, while repeated food reward decreases it (7), and the mean air-blast requirement rises rather gradually with 50 per cent punishment. With 100 per cent punishment it would rise much more steeply. Increased air blast means increased drive with consequent greater drive reduction. Thus, with 100 per cent punishment the habitual response is more rapidly strengthened, and the chances of a jump to the other side rapidily decrease. That is why if an animal placed on 100 per cent punishment does not make the alternative response relatively soon, it does not make it at all. On the other hand, once the alternative response occurs, the ensuing air-blast-drive reduction and hunger-drive reduction immediately increase the probability of its occurrence on the next trial.

3. The bimodal distribution (7) is explained by the considerations of the previous paragraph. Only while the habit strength of the alternative response is strong enough for oscillation (2, 304–321) to permit it to occur occasionally,

is it possible for the fixation to be broken. A qualitative difference of mechanism is not implied by the bimodal distribution. In some animals the alternative response simply does not chance to occur while it may.

4. The persistence of the fixated response even though food is displayed in the other window (4) is not surprising. The animal automatically makes its strongly reinforced response to the powerful air-blast stimulus because it is in that way that neural organization has been modified by the learning process. If food-stimuli do not deflect the fixated response, it is because they command a relatively trivial habit strength in this situation.

5. Trial-and-error behavior does not occur when a fixated response is punished, because, in general, trial-and-error behavior does not occur when there is a well-learned response to the stimulus concerned (in this case, air blast).

6. The effectiveness of guidance in breaking a fixation seems to be entirely due to the fact that it makes the alternative response possible, taking the place of the no-longer-effective learned tendency.

7. Fixations are more rigid than most other habits because every evocation is followed by reduction of a primary drive.

Thus, the difficulties referred to by Maier and Ellen are all explicable by learning theory. Two findings from Maier's laboratories give incidental support to a learning-theory hypothesis. (*a*) With repeated punishment the strength of fixations increases between the 8th and 16th days of experimentation (6) in a way that might be expected of a habit. (*b*) With guidance, the position responses of animals are abandoned in much the same way whether they have had frustration training or reward training (9).

Of course, if the learning-theory explanation is correct, there is nothing abnormal about the Maier fixations. But crucial experiments are needed to clear up the issue finally. One that is suggested is to give rats the usual preliminary training and then force them by air blast to jump to cards that are always unlatched and never followed by food. Learning theory would predict that this would produce fixations, while frustration theory would presumably not expect this.

Meanwhile, in another communication (19), the writer has felt justified in the view that Maier's fixation experiments do not yield examples of experimental neurosis.

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TOLMAN'S INTERPRETATION OF VICARIOUS TRIAL AND ERROR [1]

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Taylor and Reichlin (5) have recently raised three basic objections to Tolman's interpretation of vicarious trial and error (VTE) behavior. They contend: (*a*) that his sowbug schema is unrelated to his general behavior theory and is relevant only to VTE; (*b*) that Tolman's theory of VTE does not conform to the facts; (*c*) that VTE is not an aid to learning. The aim of the present article is to show that the first two contentions are false and that Tolman is therefore justified in asserting that VTE aids learning.

Relation to Other Concepts

Tolman's sowbug schema (9, 11) employs two major concepts, "progression-readiness," which propels the bug forward and backward, and "orientation-readiness," which induces the bug merely to point at stimulus objects.

As Taylor and Reichlin remark, "progression-readiness" is a complex concept. It has components referring to association, perception, and need. We first define *hypothesis* (or instrumental belief) as a three-termed association: "If stimulus S_1 is apprehended (or registered, 13) and if response of type R_1 is made, then stimulus S_2 will be apprehended." An hypothesis becomes an *expectancy* if S_1 is in fact apprehended: "S_1 is apprehended, therefore if R_1 then S_2." An hypothesis is more strongly aroused into its corresponding expectancy, the more certainly the organism identifies a present apprehended stimulus as S_1. Furthermore, the more frequently, recently, and vividly the organism has apprehended stimulus S_1, the more certainly will he identify it *as* S_1. Expectancy is the cognitive immanent determinant of behavior (7).

To come to the purposive immanent determinant of behavior, we first define *cathexis* as a two-termed association between need and goal object (7, 13): "If need N (or drive D) is apprehended then the organism will 'persist through trial-and-error' and exhibit docility in thus persisting, to get to or from a goal object of type G." A cathexis becomes a *demand* or *motive*, the purposive component of behavior, when N is in fact apprehended. The stronger an apprehended need (the sowbug's progression-tension) the greater the demand. The reader will note the correspondence between hypothesis and cathexis, and between expectancy and demand.

Behavior results from the interplay of expectancy and demand; if stimulus S_2 of the expectancy is, or is associated with, goal G of the demand, then response R_1 of the expectancy will occur and the rat will no longer remain buried in thought. A given progression-readiness is composed of hypothesis and demand. Sample hypotheses are left card —jump—food and (after learning) white card—jump—food and black card —jump—shock. Tolman assumes that total behavior is determined by the algebraic difference between the progression-readiness for the stimulus it is now facing, and all other progression-readinesses, so that the progression vector (resultant progression-readiness) to jump to the rewarded white card is

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greater if jump to the black card has been punished. Any factor reducing the resultant forward progression vector tends to permit more VTE.

"Orientation-readiness" is less easily reducible to concepts Tolman has used in other contexts, principally because he has never systematically treated the "need" for new stimuli, curiosity, and exploration for its own sake. It would appear, however, that the properties of orientation-readiness are those of the need for novel stimulation, as the latter have been described in various accounts by Lashley, Maier, Hebb, Tolman himself, and others. The present discussion is confined to vision. The properties common to the two concepts are these: (*a*) Selectivity. The organism responds principally along one stimulus dimension (or to a particular stimulus aspect) at any given time. The dimension principally responded to depends on the stimulus and on innate and acquired characteristics of the organism, and may change during a sequence of stimulus presentations. (*b*) Best View. The organism will adjust its position and orientation with respect to a stimulus object in such a way that "satiation" or "need reduction" occurs most rapidly. This almost always means bringing the object to the center of the visual field by facing it. The organism may also approach or, more rarely, retreat from the object. (*c*) Discriminability. If one of two stimulus objects remains constant, an increase in (say, positive) physical difference between it and a second object is accompanied by a nondecrease, generally an increase, in discriminability on the corresponding psychological (behavioral) dimension (s); in other words, two objects are easy to discriminate if they are physically quite different. The more discriminably different another stimulus in the field from the stimulus at present attended to or "centered," the more readily will the organism shift its attention to, seek out, or (if the organism is a sowbug) point at the other stimulus. (*d*) Need Reduction. Attending to, or centering, a new stimulus reduces almost instantly the exploratory need for the aspect of the stimulus attended to. The more frequently and recently a particular aspect of a stimulus has been apprehended, the less in amount and the faster in time is the "need reduction" it produces, because the need is already at a reduced level.

Tolman has made two special assumptions for the sowbug, both related to Best View property (*b*). (*b'*) The nearer a peripheral stimulus to the center of the sowbug's perceptual field, the greater the tendency to point at the object and center it. This assumption is confirmed by the fact that VTE frequency is higher for smaller spatial angles between stimulus objects (10). (*b''*) In short-jump situations the approach tendency due to stimulus need is negligible. This assumption may indeed be *ad hoc*, but it is not implausible and so far as the author is aware, it is not contrary to fact or inconsistent with the body of Tolman's system.

In the presence of two stimulus objects to which it is responding discriminably, the sowbug will, as a result of properties (*b*) Best View, (*c*) Discriminability, and (*d*) Need Reduction, simply oscillate, facing one stimulus then the other, until the progression vector moves it forward in the direction it is facing.

On the whole, we may conclude that the schematic sowbug lies close to the center of Tolman's theory. Indeed, "behavior-adjustment," the process underlying VTE, was an early construct of Tolman's (6), and was designated as one of the three classes of behavior determinants, along with capacity and the immanent determinants (cognition and purpose) (7). Contrary to Taylor and

Reichlin, therefore, the sowbug's characteristics are not only consistent with Tolman's system but form an integral part of it. The sowbug is merely a means for focusing Tolman's theory on a particular area of behavior, or as Taylor and Reichlin remark (5, p. 389), it is an expository device. It follows that in deriving the facts of VTE behavior we may employ concepts and principles from any part of Tolman's writings.

DERIVATION OF VTE BEHAVIOR

Taylor and Reichlin's second major contention is that Tolman's theory does not account for certain facts of VTE behavior. They concede that it accounts for two facts, that VTE is more frequent for rats *learning* an *easy* discrimination (fact 2 in their list), and that VTE is more frequent for humans making a difficult discrimination (fact 3). Fact 2 is explained largely by Discriminability, property (c), which predicts a greater tendency to oscillate with more discriminably different stimuli, and the fact that at the learning stage of the task the organism has no strong hypotheses (i.e., he is "picking up his instructions," 12) and therefore has weak progression-readiness. Fact 3 is explained by the small difference between progression-readinesses related to the two stimuli; the small resultant progression-readiness permits more oscillation, more VTE. Both progression-readinesses are small for humans in part because of low demand due to weak need-tension. There is a more important element, however, not mentioned by Taylor and Reichlin, which must be introduced to account for the same fact observed in rats (16). This element is certainty of apprehension, of "making sure of which stimulus is which" (12). If the stimulus objects are hard to discriminate, not only may the subject's hypotheses be weak but these hypotheses will be less strongly aroused into their corresponding expectancies upon apprehension of the stimuli.

Fact 1 on Taylor and Reichlin's list refers to the low frequency of VTE movements per trial in the early trials, an increase in frequency which reaches a maximum when the subject has almost stopped making incorrect choices, and a subsequent decrease in VTE frequency. Taylor and Reichlin state (5, p. 391): ". . . Tolman's theory leads to the deduction that the number of VTE movements is a decreasing function of the number of trials," in contradiction to the maximum of fact 1. Their deduction is correct, however, only if the "first" trial is taken to be the trial on which the subject begins to respond fully along the correct stimulus dimension.

The main object of Tolman's second sowbug article (11, esp. p. 379) is to show how active selectivity of perception, i.e., "discontinuity" in learning, fits into VTE behavior. As long as the subject holds an irrelevant hypothesis, say "left card—jump—food," he will not VTE in response to the brightness dimension. As he begins to "learn his instructions," to develop the general hypothesis that brightness matters, he will begin to VTE. When he has learned "what to do" and while his specific hypotheses pertaining to the individual cards are still weak, VTE frequency reaches a maximum. Then, as Taylor and Reichlin correctly state, VTE frequency decreases because of increase in the strength of specific hypotheses. VTE depends not on number of trials as such, but on the interplay of variables at different stages in learning. Tolman's schema clearly accounts for the maximum of fact 1, and Taylor and Reichlin's criticism must be rejected.

Fact 4 refers to greater frequency of VTE if the gap to be jumped is narrow (8½ inches) rather than wide (23½ inches). Tolman explains this fact (8)

by assuming that the rat cannot discriminate as well over the greater gap distance, that stimulus differences become less prominent, and therefore the rat VTEs less. Taylor and Reichlin accept the derivation but object to the *ad hoc* assumption. But fact 4 may itself be *ad hoc*, in the sense that it really depends on the rat's "myopia." If this property of the rat is not parsimonious, so much the worse for parsimony. The question is, is Tolman's assumption true or not?

Taylor and Reichlin state (5, p. 391): "Hebb (2) has presented evidence that distant visual stimuli are more effective than near ones in determining the rat's behavior." Now Hebb's evidence indicates that if rats are presented with conflicting visual cues, within jumping distance and at several feet (walls and ceiling), respectively, they tend to respond to the more distant cues, presumably because distant cues are in general less variable. The VTE behavior under discussion, however, involves cues which are not conflicting and which lie at distances of much more nearly the same order of magnitude. The fact that rats respond to stimuli several feet away does not indicate that they have the same sensitivity to stimuli 8½ inches and 23½ inches away. To the author's knowledge there is, however, no direct evidence on the question.

Tolman suggests in the same paper (8) that caution, or resistance to jumping, explains the higher frequency of VTE in jump, as opposed to non-jump, situations (cf. 4). Tolman and Brunswik (14) earlier interpret caution as preference for "indifferent" as opposed to "ambivalent" means-objects; here, jumping stand as opposed to cards. But since resistance to jumping is presumably greater for wider gaps, the rats would tend to hold back and VTE more, contrary to fact 4. The differential resistance for the two gaps must therefore be small with respect to the differential effect of gap width on discriminability and to other motivational factors.

Taylor and Reichlin's fact 5 is that there is higher VTE frequency for shock-right or shock-wrong than for no shock. In criticizing Tolman here they first misinterpret his premise and then make an incorrect derivation. Since the premise, regarding the role of VTE in learning, merits separate consideration (see below), I shall simply show how Tolman does account for fact 5. First, shock forces the subject to pay closer attention and to emphasize the difference between the two cards (7). This accounts in part for the more rapid learning in shock situations; the rat "learns his instructions" sooner. Second, it tends to make the subject more cautious (8). For shock-right, the subject is ambivalent toward the reward card and therefore holds back from both cards, meanwhile VTEing. For shock-wrong, the reluctance to be shocked for making a wrong choice will hold the subject back and VTE until one of the stimuli is identified with a fairly high degree of certainty or until response occurs for one of three other reasons specified by Tolman (11).

Further support of this interpretation comes from the fact (3) that after the subject has learned the discrimination and jumps only to the reward card, he no longer VTEs for shock-wrong (because he can now identify the cards correctly) but continues to VTE for shock-right (because he is still being punished for jumping correctly). Thus both faster learning and greater VTE in shock situations are accounted for.

We conclude that Taylor and Reichlin's objections have been refuted; Tolman's theory accounts systematically for the major facts of VTE behavior.

ROLE OF VTE IN LEARNING

Taylor and Reichlin challenge Tolman's proposal that VTE aids learning.

They state his position as follows (5, p. 391):

". . . when the animal is facing in the right direction, the stimulus 'reminds' him, so to speak, of the previous satisfactory consequences of responding to it, and that has the effect of increasing the relevant progression-readiness. Similarly, facing in the wrong direction diminishes the other progression-readiness."

Nowhere does Tolman say this. He *does* say that VTE indicates an "active selecting and comparing of stimuli" (12), which leads to better identification of the relevant stimuli and therefore to better learning (stronger hypotheses) and better performance (more accurate expectancies). Animals who learn quickly VTE more than do slow learners, and normal rats VTE more than brain-damaged rats (15). Both facts may be due to faster "satiation" from the apprehension of a stimulus object in smarter and normal rats. Except for one (apparently preliminary) experiment cited by Tolman (8), more VTE has always been reported concomitant with quicker learning and fewer errors for easy discriminations.

How can we define the real issue between Tolman, and Taylor and Reichlin? Let trial-and-error behavior be defined in brief as complete overt action resulting from expectancies having a low degree of certainty. Vicarious trial and error must occur, by definition, when the function of overt trial and error is accomplished by other means. The function of trial and error is central (the strengthening of hypotheses); the substitute, or vicarious, means may be partly central and partly overt, or completely central, if there is such a thing as thinking without motor accompaniment. Tolman uses the term VTE to refer to oscillating from side to side without forward motion. In this sense VTE could only occur in a discrimination situation.

Taylor and Reichlin, on the other hand, wish to refer only to ". . . incomplete or preparatory responses of the same type as the hesitations and false starts that may be seen when animals are learning to respond to a situation which does not call for a choice between alternatives" (5, p. 392). They interpret head movements to be not signs of vicarious trial and error in Tolman's sense, but signs of *incipient*[3] trial and error.[4]

The issue is whether VTE is predominately vicarious trial and error in Tolman's sense, or incipient trial and error in Taylor and Reichlin's sense. Let us note that Tolman's system does not exclude incipient trial and error as a mode of behavior. It is therefore maintained that Taylor and Reichlin's appeal to data gathered in a nondiscrimination situation, however material it may be in other connections, has little bearing on the present issue of VTE in discrimination situations. Since Tolman has accounted systematically for VTE results, it is incumbent upon Taylor and Reichlin to account for them more satisfactorily.

CONCLUSION

It has been shown that Tolman's sowbug schema forms an integral part of his theory of behavior; that the theory accounts for the facts of VTE, including others than those cited by Taylor and Reichlin; and consequently, that VTE may be interpreted as an aid to learning.

[3] A term suggested by Dennis and Russell (1).

[4] If Taylor and Reichlin are correct, "vicarious trial and error" is a misnomer. Perhaps we can best avoid theoretical bias by thinking of the letters "VTE" as standing for "vacillating trial and error," if we have to think of them as standing for words at all.

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SPENCE ON THE PROBLEM OF PATTERNING

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In a recent issue of this JOURNAL (8) Professor Spence has provided a long-awaited extension of his fruitful theory of discriminative learning (7) to the problem of stimulus patterning. After having been concerned for many years with the implications of a purely summative formulation, he has now turned his attention to data which clearly fall beyond the scope of the earlier theory. The occasion for the new statement was provided by an experiment on the relative difficulty of simultaneous and successive problems of discrimination (11), in the analysis of which Spence was led to consider the question of how the successive solution is achieved. A problem which requires the animal to turn to the left at the choice point of a T maze when both alleys are dark and to turn right when both alleys are bright should, in terms of a summative theory, be impossible of solution since there is no differential reinforcement in any afferent dimension.

The new formulation does not discard, but instead builds upon, the earlier summative principle, and the result is a three-level theory of perceptual development. I. In the "standard situation" (simultaneous) "the excitatory strength of the positive cue . . . becomes steadily greater than that of the negative cue" (8, p. 89) as a consequence of differential reinforcement. II. The solution of the successive problem, in which each afferent component is equally often reinforced, is dealt with in terms of the concept of *compounding*; in the T maze described above the compounds *dark-left* and *bright-right* "acquire excitatory tendencies to the response of approaching" (8, p. 90) while the compounds *dark-right* and *bright-left* do not. Such problems are more difficult to learn, and compounds will develop only "when no one of the cue members is systematically reinforced more than the others" (8, p. 90). III. A still higher level of perceptual organization—*transverse patterning*—appears in problems which "require that the animal on each trial respond or not respond (by approaching) to a particular figure depending on what the other figure [is]. . . . According to the theoretical view proposed here, response on the basis of such cue-cue relations . . . would take place in nonarticulate organisms, but only under conditions that would not permit learning on the basis of a single reinforced component or some simpler type of cue-position pattern (e.g., the type of patterning involved in the successive problem)" (8, p. 92). Spence thus proposes a hierarchical conception of perceptual development; each level appears in the context of a specifically defined problem, and higher-level organizations develop only when the conditions for the development of lower-level organizations are not met.

While this extension of the earlier theory, which brings it into closer correspondence with the realities of perceptual organization, may be welcomed as a step in the right direction, considerable evidence is available to suggest that it has not been carried far enough. The principal weakness of the new formulation is to be found in the restricted role which is assigned to what Spence has called "transverse patterning." In the most general sense this term refers to the effect of variation in the grouping or configuration of stimuli in a multiple-

choice discriminative problem (e.g., the arrangement of cards in a two-window jumping apparatus) when the reinforcement of specific components or compounds is equated. This grouping effect is not limited to the rather specialized kind of problem which Spence describes, which suggests that what the older summative formulation requires is not supplementation but a more thoroughgoing revision.

Transverse effects in "simultaneous" problems. In a recent experiment Saldanha and Bitterman (6) trained rats in the jumping apparatus to discriminate concurrently between two pairs of cards —two black-and-white vertically striped cards differing in stripe width and two homogeneous gray cards differing in brightness. For Group A the positive stripe was paired with the negative stripe and the positive gray with the negative gray (Problem A), while for Group B the positive stripe was paired with the negative gray and the positive gray with the negative stripe (Problem B). Although the differential reinforcement of afferent components in this Level I problem was the same for both groups, a transverse effect showed up clearly. The animals of Group A mastered Problem A and then Problem B before the majority of animals in Group B mastered Problem B. These results were accounted for in terms of the opportunity which was afforded by Problem A for comparison of the stimuli to be discriminated. Similar results have been obtained by Coate (1) in the context of a continuity experiment.

Still a third experiment on the effect of pairing was conducted by Elam and Bitterman (2). Rats were trained in a jumping apparatus to discriminate between black-and-white striped cards differing in thickness under conditions which provided experience with both horizontally and vertically striped cards. When the thickness discrimination had been mastered, the animals were trained to discriminate between horizontal and vertical stripes irrespective of thickness. For one group the two thin stripes and the two thick stripes were paired, while for a second group the cards were paired in such a way that a difference in thickness was present along with the difference in orientation (i.e., thick-vertical with thin-horizontal and thin-vertical with thick-horizontal). The relational presentation of *irrelevant* components *retarded* discrimination.

In a second series of experiments by Teas and Bitterman (9) and by Turbeville, Calvin, and Bitterman (10) further evidence of a grouping effect in Level I problems was obtained. These studies were designed to investigate what may be called the *two-situational* problem. Suppose that animals learn concurrently a discrimination between two vertically striped cards differing in thickness and between two gray cards differing in brightness. The stripes and grays are presented as pairs and the lateral arrangement of each pair is varied systematically. The problem thus presents *four* card arrangements to the animals—the two lateral arrangements of each pair. In the corresponding two-situational problem each pair is presented in only one lateral arrangement—e.g., light gray on the left and dark gray on the right, thick stripes on the left and thin stripes on the right—with the positive card of one pair on the left and the positive card of the second pair on the right. In both problems, therefore, one of the grays is consistently reinforced while the other is consistently punished, and each spatial component is randomly reinforced and punished. According to both the earlier and the later versions of Spence's theory the two problems should be functionally equivalent. The results show, however, that the two-situational problem is significantly easier than the four-situational problem and that there is little transfer from the first problem to the second.

These results demonstrate that, even in the context of a problem which provides differential reinforcement of afferent components, different levels of perceptual organization may emerge depending on the grouping of the stimuli.

A third series of investigations by MacCaslin, Wodinsky, and Bitterman (5) on the process of stimulus generalization bear on the problem of transverse patterning. In one of these experiments a group of animals was trained to discriminate between horizontally and vertically striped cards (vertical positive) and a second group was trained to discriminate between the vertically striped card (positive) and a black card. The groups were equated for number of reinforcements on the positive card and then trained to discriminate it from another vertically striped card (negative) differing in thickness. The performance of the first group was superior to that of the second, demonstrating that the context in which reinforcement is given determines to an important extent the nature of perceptual development. An analysis in terms of components leads to precisely the opposite prediction on the basis of the inhibition which should be generated from the horizontally striped card of the first problem. Similar results were obtained in other experiments of this series.

Transverse effects in successive problems. An experiment by Wodinsky and Bitterman (12) demonstrates grouping effects in a problem which is possible of solution on the basis of cue-position compounds alone. Animals were trained in a three-window jumping apparatus on a problem involving black, white, and vertically striped cards. One group was reinforced for jumping to one window when three black cards appeared, to a second window when three white cards appeared, and to the third window when three striped cards appeared (e.g., *B*BB, W*W*W, SS*S*, where the positive cards are italicized). For the second group the card-position relations were identical except that the cards were grouped differently (e.g., *B*SW, S*W*B, WB*S*). As each animal in each group reached criterion on its problem, it was shifted to the problem of the other group. According to Spence's latest formulation, the two problems, which involve identical compounds, should be functionally equivalent; arrangement should make no difference because the problems can be solved on the basis of compounding alone. The results, however, show a clear transverse effect. The first problem proved to be more difficult than the second and transfer from each problem to the other was quite incomplete (initial responses were about 60% correct as compared with a chance level of 33%).

Theoretical and experimental constriction. Although these experiments clearly demonstrate the operation of transverse effects in first- and second-level problems, it may be anticipated that Spence will not consider them relevant to his theoretical position. For one thing, the experiments were performed with the jumping apparatus which involves the use of punishment for errors, a condition which Spence has "specifically avoided" (8, p. 89). Furthermore, in some of the experiments the correction method of training was utilized, although Spence has concentrated upon results obtained with the noncorrection method. Finally, there is reason to expect that Spence may regard the experimental designs as too complex. In discounting the results of Weise and Bitterman he remarks, for example, that "it is difficult to interpret the very complex type of discrimination set-up they employed. The simple discrimination situation is sufficiently difficult to deal with theoretically without adding all of the problems that arise as the result of the serial nature of the multiple-discrimination set-up along with the fact that it involves a gradient

of reinforcement" (8, p. 91).[1] Similar reasons might be advanced for discounting the experiments here described.

Now it is easy enough to protect a restricted theory by the design of restricted experiments, and it is easy to emphasize the limitations of such a theory by the design of broader experiments. The fundamental problem concerns the relative value of the two approaches. As Leeper (4) has noted, it is possible to take the position that research under restricted conditions will at least reveal the fundamental processes at work under those conditions, although the principles thus derived may have to be supplemented when the scope of research is broadened. Leeper himself believes that the principles derived from research under restricted conditions are likely to be superficial. "Such research," he maintains, "is not conducive to the discovery, in any deep and significant sense, of the fundamental processes at work even within a limited area" (4, p. 489). It might be well if this position were given serious consideration.

At other points in Spence's presentation restrictions seem to be brushed aside and his principles seem to acquire a more general flavor. This tendency is especially evident when he denies that "the type of patterning that the Gestaltters and Gulliksen and Wolfle are talking about" may be evidenced in problems which can be mastered on the basis of differentially reinforced components or compounds (8, p. 92). It may be maintained on the basis of the evidence here reviewed that such effects do occur under certain experimental conditions. While the "complexity" variable is difficult to evaluate, the influence of the correction methods and punishment for errors should be studied in subsequent experiments. It may be anticipated that the results of such experiments will demonstrate the need for a more fundamental modification of Spence's theory than has yet been proposed.

[1] The experiment in question and the one by Spence provide the only comparisons of simultaneous and successive problems to be found in the literature. Spence should not have been "surprised" by the failure of Weise and Bitterman to consider Lawrence's data. Lawrence made no directly comparable studies of the two kinds of problem. Results identical with those of Weise and Bitterman (obtained with a modified jumping apparatus) will soon be published (M. E. Bitterman and Jerome Wodinsky, Simultaneous and successive discrimination, *Psychol. Rev.*, in press).

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FRUSTRATION AND THE QUALITY OF PERFORMANCE: II. A THEORETICAL STATEMENT

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What is the effect of frustration on the quality of performance? There appears to be a dual tradition in the writings of psychologists and others who have given attention to this problem.

First, there is a tradition that frustration leads to improved quality of performance. Dewey's often cited account of why thinking occurs stresses the role of a problem or difficulty as the occasion for creative intellectual activity (8). Difficulty in such a situation often is an instance of frustration.[1] In more general accounts of the psychology of adjustment, unreduced tension is shown as giving rise to various forms of adjustment, of which some may be of high intellectual quality (25). On the level of society as a whole there are notions such as Toynbee's (26)—that the protracted existence of a challenge, often in the form of difficulty in meeting the needs of bare subsistence, is the condition for the joint constructive activity that produces a new civilization.

Second, there is also a tradition that frustration leads to lowered quality of performance. This is perhaps the more apparent part of the thesis of psychoanalysis and psychology of adjustment, since, on the whole, adjustments of poor quality to frustration have received the greater attention from therapists. This tradition is also evident in much of the discussion about the disorganizing effects of emotion (as reviewed, e.g., by Leeper [15]), inasmuch as emotion is often produced by frustration. Barker, Dembo, and Lewin's study of frustration and regression (2) is often cited in simple confirmation of this tradition, to the neglect of the rest of its content. Most recently this tradition is represented in Maier's systematization of the effects of frustration (20), as most of the effects he deals with would doubtless be considered to be of poor intellectual quality.

There is, then, an apparent conflict of belief in this matter. Indeed, the conflict appears strikingly in some general textbooks in psychology. In a chapter on thinking and reasoning frustration is viewed as the condition for more organized behavior, and in a chapter on emotion it is viewed as the condition for less organized behavior. The failure to use a common term such as frustration in the two chapters apparently permits the contradiction to go unnoticed.

Is this apparent contradiction due merely to failure to appreciate the role of severity of frustration, minor frustrations leading in fact to an improvement in quality of performance and major frustrations to the opposite, as might be inferred from the settings in which these contrary effects are often discussed?

[1] We are using *frustration* in a broad sense to refer to prevention of a person's direct progress toward a goal, not wishing to prejudge by definition the importance of various distinctions that can be made among the variety of events that fit this definition. We heartily agree with Brown and Farber's emphasis on the need to distinguish sharply between this definition of frustration and its definition as referring to a state of the organism (5, p. 480). But we feel it more useful to apply the term to the *event* of prevention of a person's progress toward his goal than to a *state* which may in some cases be inferred from the event.

Presumably not in any very uniform way, else why would anyone swear when he stubbed his toe, and how could any prisoner ever carry through successfully an ingenious plan for escape?

The greatest advance toward resolving this contradiction has been made by Barker (1) and by Barker, Dembo, and Lewin (2). By drawing upon their contributions, upon other aspects of psychological theory, and upon evidence obtained in a variety of pertinent studies, we hope to advance still further towards an understanding of the factors which influence the direction of change in quality of performance that results from frustration.

We have found it convenient to deal with three problems which it is useful to separate for purposes of analysis:

I. Effects of frustration in one activity upon the quality of performance in a second activity.

II. Effects of frustration in one activity upon the quality of performance in that activity.

III. Effects of frustration upon the quality of a person's behavior as a whole.

The three sections of this paper will be devoted to these three problems in turn. For the sake of brevity only one of these problems—the second one—has been selected for detailed treatment.

I. Effects of Frustration in One Activity upon the Quality of Performance in a Second Activity

The well-known experiment of Barker, Dembo, and Lewin is presented by those authors as dealing with a generalized effect of frustration upon the constructiveness of a person's behavior as a whole (2, p. 46). Actually, a critical analysis of the procedures and results indicates that it can only be said with certainty to deal with the effects of frustration in one activity upon the quality of performance in a second activity. The activity frustrated was children's play with a highly attractive set of toys; the second activity, in which quality of performance was measured, was play with a much less attractive set of toys. The theoretical discussion by Barker, Dembo, and Lewin, like their data, is most directly relevant to the problem of this section.

In discussing this problem Barker (1) and, less sharply, Barker, Dembo, and Lewin (2) make a definite contribution to an understanding of the factors which determine whether frustration will lead to a better or poorer quality of performance. The suggestion they make about frustration in relation to poorer quality of performance we would rephrase as follows: frustration of one activity will produce lowered quality of performance in a second activity to the extent that it leads to the making of responses which interfere with the responses of the second activity. Barker, Dembo, and Lewin minimize the role of this sort of hypothesis in explaining their results. We have shown in a previous paper (6), however, that their results actually support this hypothesis very strongly; and we feel that this is the most important empirical contribution of their study.

The opposite effect, improved quality of performance, is ascribed by Barker, Dembo, and Lewin to what we would call an increase, resulting from frustration of one activity, in the strength of drives which support the second activity. Barker (1) suggests three conditions under which such drives are likely to be strengthened in a way which results in increased quality of performance. We would rephrase them as follows:

1. When the second activity can be and is motivated in part by the original, unreduced drive which had been motivating the

frustrated activity, so that the second activity functions as a substitute for the first.

2. When frustration-produced drive leads to an attempt to escape from reminders of the frustrated activity, and preoccupation with the second activity is the mode of escape hit upon.

3. When the person was previously especially unmotivated with respect to the second activity, for it is then supposed that quality of performance may be favorably influenced by increased drive more than it is unfavorably influenced by interference.

These all seem to be significant suggestions, and in each case allied fields of research could provide evidence that indirectly supports their plausibility. They have not, however, been tested systematically in research on quality of performance, though they are drawn upon by Barker, Dembo, and Lewin in interpreting the behavior of individual subjects who in their experiment showed an increase instead of a decrease in constructiveness after frustration (2, pp. 179–186).

This contrast between the effects of interference and of increase in relevant drive, resulting from frustration, seems to us of fundamental importance, though it leaves many questions unanswered. This same contrast will be made in connection with the second problem, to be considered in the following section of this paper. Other points to be made in the following section can also be applied, with modification, to the present problem, but we shall discuss them explicitly only with reference to the second problem. There remains to be made here, however, a special point about the interference effect of frustration upon a second activity, a point which is distinctive for the problem of this section and essential for putting into proper perspective the role of frustration here.

The point is this: Frustration of the first activity may, *in comparison with active pursuit of the first activity*, actually increase the quality of the second activity by reducing the amount of interference with it. This is particularly likely to be true if the two activities are essentially alternatives of which the first activity is the preferred or dominant one. For if in this case the preferred activity is being pursued without frustration, all the overt responses which make it up are present to interfere with possible pursuit of the second activity. If, on the other hand, the preferred activity is thoroughly frustrated, there may remain, as possible sources of interference with the second activity, only implicit tendencies to return to the preferred activity. Interference arising solely from implicit tendencies, from thoughts, seems likely on the whole to be much less severe than interference arising from successful overt pursuit of a dominant activity. We suggest that one aspect of the Barker, Dembo, and Lewin experiment can probably be viewed in this light, though the design of their experiment does not permit our suggestion to be tested. We can only illustrate our meaning by suggesting a variation of conditions which was not actually used in their experiment.

The constructiveness of children's play with relatively unattractive toys was initially measured in a free-play period, with no other toys in sight. Later, the constructiveness of their play with these same toys was measured during a frustration period, in which the children had just been interrupted in play with more attractive toys and these more attractive toys remained in sight behind a wire barrier.[2] The constructiveness of

[2] For the purposes of the general point under discussion it should be noted that the play with the attractive toys is here regarded as the first activity, and the play with the unattractive toys is regarded as the second activity.

play with the unattractive toys was lower during the frustration period than it had been during the free-play period; but still, it was an activity of considerable constructiveness, or quality. Our contention is that the constructiveness of play with the attractive toys would not have been as high as it was, had it not been for the frustration arising from inability to play with the attractive toys. For, suppose that instead of being frustrated, the children had been allowed to continue play with the more attractive toys, the unattractive toys being put off by themselves in another part of the room. What, in this case, would have been the quality of performance in the second activity, i.e., interaction with the unattractive toys? We would predict that it would fall into a very much lower level still—that it would be largely confined to glances and sporadic beginnings of play, rapidly interrupted by return to the more attractive toys.[3]

Frustration of a preferred activity, then, may produce for a second activity a degree of interference which is intermediate—intermediate between the greater interference which would have occurred in the absence of the frustration and the lesser interference which would have occurred in the total absence of the preferred activity.

It is none the less true that frustration of one activity may constitute a genuine source of definite interference with a second activity. This fact seems to be clearly demonstrated in two experiments which were intended to deal with another problem, that of repression. In both cases there was, as it happened, no question of a more preferred and a less preferred activity; the instructions of the experimenter simply required the subject to work at two activities successively, and it was possible to observe the effect of frustration in one activity upon the quality of performance in the other activity. Sears (24) found that frustrating subjects in their attempts to do well at a card-sorting task led to a lowered quality of performance in a learning task. Zeller (27) found that frustrating subjects in their attempts to do well at the Knox Cube Test also led to a lowered quality of performance in a learning task. In both instances it appears that frustration in one activity led to internal responses (worry, for example) which interfered with maximally effective prosecution of a second activity.

It is probable that for Barker, Dembo, and Lewin's subjects as well, there was interference genuinely arising from the frustration. This is suggested by the fact that the overt responses of their subjects included attempts to escape from the situation, a response which doubtless contributed to the total interference and appears to be a response specifically to the frustration. But the total interference was probably much less than it would have been without the frustration.

In sum, then: Where quality of performance is lower than might be expected, and this lowering appears to be connected with the course of other activities, frustration of other activities is one possible source of interference; but successful pursuit of other activities may be a more important one. The college student who is frustrated in his attempts to arrange a date for the evening may not learn his German vocabulary that evening as well as he could; but it's a good bet that he'll learn it better than if he had had a date.

[3] This prediction, as applied to the Barker, Dembo, and Lewin experiment, is complicated by the fact that children could integrate the two sets of toys in a single play activity. For our point to be made, one must suppose that the rules of the situation did not permit this integration—a restriction which, for many situations to which one would wish to generalize, is imposed by the very nature of the activities.

II. EFFECTS OF FRUSTRATION IN ONE ACTIVITY UPON THE QUALITY OF PERFORMANCE IN THAT ACTIVITY

In dealing with the effects of frustration in one activity upon the quality of ongoing performance in that activity itself, we shall organize our discussion under five main headings. These represent five kinds of process or event which may influence the effect that frustration has on the quality of performance. This analysis has been difficult, because the several processes or events are closely interconnected and in many an instance would all be operating at once. We believe, however, that this sort of analysis is useful for reaching an understanding of the effects we are dealing with.

A. Extinction of the initial response to the situation

When a person is frustrated in some activity, the situation to which he is responding is thereby somewhat changed. The extent to which it is changed, however, varies, and in some instances it may be useful, in predicting his response, to consider the situation to which he is responding as essentially the same as it was before the frustration. Where this is a useful approach to make, Hull's concept of the habit-family hierarchy (12), expressed in a somewhat more general form, suggests the importance for our problem of the extinction of the initial response to the situation.

A person may be conceived of as having, in any specific situation, tendencies to make various response sequences which may all potentially lead to the goal towards which he is oriented. These various response sequences may be thought of as a hierarchy, the various members of which differ in habit strength (that is, in the strength of the tendency for them to be evoked). The sequence for which the habit strength is initially strongest will be the one first evoked. If the resulting activity is frustrated, its habit strength is diminished by the process termed extinction. With persisting frustration, its habit strength may be reduced below that of the other members of the hierarchy. At this point the other response sequences in the hierarchy will begin to be evoked. The effect of frustration upon the quality of performance in this case, then, will depend upon the relative quality of the initial response sequence which is extinguished and of the other response sequences which are then evoked instead.

On the whole, it seems likely that the initial response sequence will be the sequence of highest quality in the hierarchy. The reason for this expectation is that the response sequence of highest quality is likely to have been the most strongly and consistently rewarded in similar situations in the past, and thus to have become the response sequence of greatest habit strength and the one first to be evoked.[4]

Several experiments on frustration appear to illustrate this effect of extinction of initial response upon quality of performance, though in each case it is quite likely that other processes to be considered later were also influential. Three experiments can serve as particularly apt examples. In each case the subject was required to engage in some more or less intellectual task. In an experiment by Sears (24), and in one by McClelland and Apicella (16), the task involved card sorting; in an experiment by Postman and Bruner (22), the task consisted of attempting to perceive and report words as their exposure time was gradually increased from a subliminal value. In a subject faced with any of these tasks, there is evoked a general mode of responding which is likely to be

[4] We are indebted to Dr. Gregory A. Kimble for suggesting this point to us.

about the most adaptive of which he is capable. But in each of these experiments, as the subject responded and continued to respond in this adaptive manner, the experimenter withheld the normal reward of knowledge of satisfactory performance, and instead told the subject he was failing miserably, doing worse than anyone else, etc. The lowered quality of performance which then appeared in each of these experiments may well have been due primarily to the extinction, resulting from nonreward, of the subject's adaptive response tendencies, together with the fact that for this situation the subject did not have any alternative response tendencies which would at all rival the initially dominant one in the quality of performance to which they would lead.

The effect of extinction of the initially dominant response tendency is not, of course, necessarily a lowering of quality of performance. The order of response sequences in the hierarchy for the given situation may be determined by generalized effects of learning which took place in a previous situation (or situations) which was appreciably different from the present one. In particular, the previous and present situations may differ in the quality of performance which would be judged to characterize particular response sequences if evoked in those situations. Thus the present situation may first evoke a response sequence which was of high quality in former situations but of low quality in this situation; extinction of the tendency to respond with this sequence *may* in that case lead to the evocation of a response sequence lower in the hierarchy but of higher quality in this situation.

B. Situational changes

In the preceding section we considered certain implications that follow when the frustrated person may be considered to be still responding to essentially the same situation. In this section we turn to certain implications which follow when the character of the frustrating circumstances is such that the person must be considered to be now responding to a situation very different from that which preceded the frustration. We shall not deal here with the fact of frustration itself as a new element in the situation to which the person is distinctively responding; this matter we shall discuss in Section D. We shall deal here simply with specific changes in the situation which are inherent in the specific manner by which the frustration is brought about.

The point we are concerned with here is this: One effect of frustration is to alter the person's situation in such a way that behavioral possibilities are changed, and this alteration has implications for the possible quality of the person's performance.

On the one hand, frustration may alter the situation in such a way as to render impossible any responses of high quality directed at the original goal. There is an approach to this condition in the Barker, Dembo, and Lewin experiment (2). The highly constructive behavior of complex play with the desirable toys was rendered impossible by making those toys completely unavailable to the child. If the constructiveness of behavior in relation to the goal of playing with those inaccessible toys was characteristically reduced by frustration (no systematic evidence was in fact collected on this point), was it not largely because this highly constructive behavior was made impossible and no other equally constructive behavior in relation to that goal was possible for most of the children? Parallel examples from nonlaboratory situations come readily to mind. For the man whose beloved marries someone else, the formerly most constructive behavior of striving by appropriate means to gain

her affection is now impossible if he accepts the morality of this society. Indeed, the situation is now so changed that there may be no possibility at all of constructive behavior directed at his original goal; if he is still so strongly driven toward this goal as to make some kind of response in that direction, it must of necessity be of poor intellectual quality, as, for example, wish-fulfilling fantasies, or various nonconstructive social acts.

On the other hand, frustration may alter the situation in such a way as to make possible the achievement of the goal by acts of higher intellectual quality than were previously possible or appropriate. The man who is digging a hole with his spade, in order to plant a tree, has no more constructive behavior open to him than the simple routine of digging; though this is the most efficient and adaptive behavior under the circumstances, it is not of very high intellectual quality. But if the handle of the spade breaks, and he is thus frustrated, more complex and more constructive behavior, of higher intellectual quality, now becomes possible and indeed essential as a means to the original goal. The skill of digging with a spade must now be integrated with the skill of shaping a new handle, or with the social skills involved in borrowing or buying another spade in a much more complex sequence of behavior leading to the original goal. This sort of effect may be seen in the Barker, Dembo, and Lewin study if one looks solely at the means by which the child achieved, or might have achieved, contact with the desirable toys. When they were freely available to him, he simply approached and touched them. When a barrier was interposed, the only behavior that might possibly have led him to these toys was a much more complex sequence of influencing the experimenter, though, as it happened, it had been predetermined that even this should not be successful.

C. Quality of the responses available for performance

In sections A and B we have shown that the elimination of one response, as a result of frustration, may influence the quality of performance. In section A, we considered the elimination of one response through extinction. In section B, we considered the elimination of one response because of the removal of some kind of environmental facility or support which is essential for its performance. Just how this elimination of one response will affect the quality of performance depends, of course, both upon the quality of the eliminated response and upon the quality of the other responses which then come to be made. We must now consider explicitly, therefore, the question of what variables influence the quality of the responses available in the person's repertoire and likely to be made if frustration eliminates the initial response.

In the case we have dealt with in section A, where the person may be considered as responding to essentially the same situation before and after frustration, we have already suggested that Hull's concept of the habit-family hierarchy provides a useful theoretical schema for dealing with this problem. The quality of the new behavior resulting from frustration would be predicted from the quality, as responses in this situation, of the responses next to the initially dominant one in the hierarchy. Actual application of this schema, of course, requires measurements both of quality and of habit strength of response sequences. Such measurements are certainly possible for complex human behavior, and have been made in connection with other problems. With reference to studies already done which are directly relevant to this problem, how-

ever, the schema can only be applied by using gross judgments of great differences in quality and habit strength between the initial response sequence and other response sequences, as in our interpretation of the experiments we cited in Section A.

In the case we have dealt with in Section B, where the situation to which the person is responding must be considered as radically changed, the same theoretical schema of Hull's may be considered as sometimes applicable. Here the quality of performance after frustration would be predicted from knowledge of the quality represented by the response sequence ***highest*** in the habit-family hierarchy for this new situation. We know of no research studies, on just this point, to which this mode of analysis is readily applicable. It is obviously applicable, however, to incidents of everyday life. Imagine a person driving his car to work who is frustrated by a flat tire, which radically changes his immediate situation. The quality of his response, e.g., swearing and sulking vs. changing the tire or calling a repair man, seems likely to be influenced by what particular response tendencies to this changed situation have become dominant as a result of his previous experience in similar situations. This sort of analysis should also be useful in leading to systematic research.[8]

Regardless of how the elimination of the initial response is brought about, however, the concept of the habit-family hierarchy is not by itself adequate to deal with all cases. For in many cases the initial response sequence is replaced, not by some other response sequence which has a predictable habit strength resulting from previous reward or nonreward in similar situations, but rather by some novel response sequence which has never previously been performed by the person in any situation.

Now, under these circumstances, the problem of predicting the effect of frustration upon quality of performance becomes the specific problem of predicting whether a person, in the face of frustration, will produce novel responses and whether these novel responses will be of high or low intellectual quality. This specific problem is one to which a considerable body of scientific research is relevant. Relevant research is indeed so voluminous that we cannot hope to review it here. All we wish to do here is call attention to its relevance to the problem of frustration, for most of this research has been performed, and has been discussed, in contexts far afield from the dynamics of frustration reactions.

First of all, there is research on intelligence as an organismic variable which influences the person's reactions in a variety of situations. If intelligence tests measure so broadly relevant a variable as is often hoped, that variable should be highly useful in predicting the quality of a person's response to frustration—in predicting, in other words, the likelihood that a frustrated person will hit upon a novel response of high quality rather than persisting in an unsuccessful response or making novel responses of poor quality.

Second, if this first point is correct, research on determinants of intelligence is also relevant to the present problem. If heredity, nursery-school training, institutionalization, intellectual character of the home environment, etc., influence general intelligence, they should influence the likelihood that the frustrated person will make a novel response of high quality.

[8] The only close parallel in systematic research that we know of is in a study by Davitz (7), which is concerned with the problem of section I of this paper. He frustrated children in an enjoyable activity of watching movies and eating candy. Their subsequent responses in a free-play situation were found to be influenced, in a way relevant to quality of performance, by previous training in a similar situation.

Finally, research on factors in the immediate situation which influence the adequacy of reasoning and problem solution is relevant to the present problem. Such research has not ordinarily been formulated as dealing with frustration. When we speak of frustration, we ordinarily think of a person as at first anticipating steady progress toward his goal, and at a later point encountering a barrier. In experimental studies of reasoning and problem solution, on the other hand, the barrier is generally present at the outset; the subject is asked to orient himself toward a goal which is obviously difficult to attain. But the determinants of quality of response under these special circumstances should certainly help to illuminate also response to problem situations as they arise in the form of frustration in normal life.

As this research on situational variables in problem solution is less widely known than that on intelligence and its determinants, it may be pertinent here to cite examples. A series of experimental studies by Maier provide apt examples. Maier demonstrates that the likelihood that a subject will make the novel and correct response to a problem situation is influenced by demonstration to the subject of part-responses which are required for it (18), by giving to the subject a particular kind of part-response which Maier terms "direction" (17), and by instructions designed to establish a general set towards flexibility of response (19). Such variables as these seem clearly applicable to understanding the quality of response to frustration in everyday life.

Much of the research on the availability of novel responses of high quality has been done in a strictly empirical context. This research is too diversified for us to attempt here any theoretical integration. We would like to point out, however, that the same kind of behavioristic analysis which we apply elsewhere in this paper is applicable here too. Examples of its application may be found in a theoretical paper by Hull (13) which discusses an experiment by Maier on "reasoning" in rats, and in Dollard and Miller's recent discussion of problem-solving behavior in human beings (9). That such an application of systematic theory may be fruitful for research in this area may be illustrated by recent studies by Birch (4) and by Gladstone (10), which demonstrate an influence of learning on the availability of responses for problem solution.

D. Habits of responding to frustration

We have so far considered the person as responding to the situation in which frustration is occurring, but not as responding to the fact of frustration per se. But the occurrence of frustration is, of course, itself a distinguishable aspect of the situation to which the person may respond distinctively. A person might conceivably have general habits of responding to all frustrations, or he might have more specific habits of responding to particular classes of frustration which for him were distinctive. The possible responses which might, in various individuals, have come to be elicited by the cue of frustration, are of course innumerable. We propose to call attention here to several classes of response which appear to have a special relevance for the influence of frustration upon quality of performance.

1. *Persistence vs. withdrawal.* Persistence in striving for the goal, in the face of frustration, is a response which keeps the individual in the situation and makes possible the emergence of novel responses of high intellectual quality, though whether such responses do in fact emerge will then depend upon such variables as those considered in section C. The degree of persistence appears to be in part determined by habits of response to frustration. Grosslight and Child (11) showed, for one experimental situation, that subjects who had been sub-

jected to frustration in the experiment and rewarded for persistence, subsequently persisted much longer in the face of continuous frustration than did subjects who had experienced only success until the time of continuous frustration.[6] In the same study tentative evidence was found that the first group of subjects, as a result of their persistence, were more likely to make novel or creative responses of a sort which under many circumstances would lead to a removal of the frustration. The second group of subjects, on the other hand, were more likely simply to withdraw from the situation or confine their responses to mere staring.

2. *Interfering responses.* Another difference among persons in their habits of responding to frustration has to do with their tendency to make responses which interfere with effective pursuit of the original activity and thus lower its quality. Thus Waterhouse and Child [7] used a questionnaire to measure the extent to which individuals habitually respond to frustration with potentially disruptive reactions, such as aggression, self-blame, and self-justification. They found that people scoring high on this personality measure, when subjected to experimental frustration, showed a lowered quality of performance; people scoring low on this personality measure, on the other hand, when subjected to the same experimental frustration, actually showed an improved quality of performance. A closely parallel finding was obtained by Mandler and Sarason (21; cf. their Predictions 4 and 5) in a study using a different kind of intellectual performance and a different measure of tendency to make interfering responses, a questionnaire designed to measure degree of anxiety as a response to examination situations. This finding is still further confirmed in another experiment by Sarason, Mandler, and Craighill (23). We may tentatively conclude that strong habits of responding to frustration, or to the anticipation of frustration, with responses which interfere with a complex intellectual activity tend to lower the quality of such activity when it must be performed in a frustrating situation.

3. *Drive-producing responses.*[8] Among the responses a person may make to frustration are internal responses which create or strengthen drive states. Indeed, some of these drive-producing responses are among the interfering responses we have mentioned in the preceding paragraph. Drive-producing responses, however, have two special properties in relation to the present problem.

(*a*) Certain drive states produced in response to frustration may operate to increase the motivation supporting the goal-oriented activity and thereby to improve the quality of performance. Individuals who have habitual tendencies to react to frustration with responses which create or strengthen these particular drive states would then improve their performance in the face of frustration. There appears to be no published research which provides clear evidence directly relevant to this notion, or which would indicate what particular drive states operate in this way, but this gen-

[6] A finding, of course, parallel to the typical outcome of experiments on partial reinforcement using traditional conditioning techniques (14).

[7] Frustration and the quality of performance. III. An experimental study. *J. Pers.*, 1953, 22, in press.

[8] Brown and Farber (5) have recently published an article which, while not focused on the problem of quality of performance, is highly relevant at this point to our treatment of this problem. Their "'emotional' interpretation of frustration behavior" might be regarded in large part as a much more thorough attack on the problem we deal with under the label of "drive-producing responses." We differ from them in viewing an emotional interpretation, and what they call nonemotional interpretations, which would include most of the rest of our treatment, not as alternative approaches (5, p. 480) but as two aspects of theory which need to be put together for the prediction of behavior.

eral notion appears to us to be a useful guide to future research.

(*b*) Quality of performance is likely to be greatly influenced not only by the drive states created by frustration, but also indirectly by other responses which are evoked by those drive states. The individual's habits of responding to drive states—in particular to the drive states likely to be evoked by frustration—thus are crucial in determining the effect of frustration upon the quality of his performance in the original activity. Among the drive states likely to be evoked by frustration are states of intense general emotion. These emotional states provide an apt example to illustrate the point we wish to make here.

Psychology textbooks often refer to the disorganizing effects of severe emotion (**15**). Undoubtedly severe emotion does often have a disorganizing effect and thus reduces the quality of performance in the face of frustration. In part this may be because the emotional responses themselves are to some extent incompatible with the ongoing instrumental activity. But in even greater part the disorganizing effect may have to do with responses *to* the emotional state. A typical person in our society is likely to have well-established tendencies to react to strong emotion with various responses—such as withdrawal from the emotion-arousing situation, close attention to the emotional experience, worry, expressive behavior such as swearing and gesturing—which all tend to interfere with efficient pursuit of the original goal-oriented activity. We would suspect that persons with a different habit structure might react to the same emotional states in themselves with a higher, rather than a lower, quality of performance. This appears to be the assumption underlying certain aspects of military training and implicit in the belief that seasoned troops are more dependable than inexperienced ones—the assumption that training can modify the way a person responds to an intense emotional state, indeed can modify it so radically that intense emotion may come to have an organizing rather than a disorganizing effect on behavior.

E. *Situational and task variables in relation to the fact of frustration*

In section D we have shown that the person's habits help determine whether his response to the fact of frustration will be such as to improve, or such as to detract from, the quality of his performance. In this section we wish only to point out briefly that the person's response to the fact of frustration will also be influenced by a variety of situational and task variables. The same kinds of response to frustration remain pertinent here.

Differences in instructions or in initial set given by the situation, for example, may influence the likelihood that frustration will evoke persistent striving or, on the other hand, withdrawal. Various specific circumstances in the situation may help determine whether frustration evokes responses which interfere with the original activity, and what effect it has on drive states and on responses to these. The extent to which heightened drive can lead to improved performance, and the extent to which other responses are incompatible and produce interference, may vary with the exact nature of the task or activity in which quality of performance is being judged. In all these ways, then, the kinds of responses we have considered in section D are relevant to the effect of frustration on quality of performance, but relevant not only as a function of the person's habits but also as a function of situational and task variables.

III. EFFECTS OF FRUSTRATION UPON THE QUALITY OF A PERSON'S BEHAVIOR AS A WHOLE

In the analysis we have presented thus far, frustration and its consequences may

play a very unimportant part in the total life of the person. Yet there is nothing about the analysis we have presented that restricts it to such cases. We wish to illustrate here the applicability of the analysis to cases of more pervasive effects on quality of behavior, though detailed application to any one problem is beyond our present intent.

One sort of case in which the question of a pervasive effect of frustration on quality of performance arises is the situation where the frustrating circumstances are such as to interfere with not one, but a great many of the person's strivings. Several such situations have been studied by psychologists and other social scientists, e.g., internment in a concentration camp, unemployment, and subjection to severe acculturation pressures. As a single example, to illustrate the applicability of our analysis, we refer to Bettelheim's study of concentration camp inmates (3).

Where very widespread frustration is imposed by the environment, as in concentration camps, it seems clear that the elimination of old responses, partly through impossibility of performance and partly through extinction, is a first determining factor. But with old responses eliminated there is a wide range in the quality of the new responses that appear. The availability of specific responses to this situation will be one potent influence; an example is the role that Bettelheim's scientific background must have played in enabling him as an individual to make a highly constructive adjustment to a concentration camp, through reacting to the situation in part as an intellectual challenge (3, pp. 422–424). Another potent influence is likely to come from the individual's habits of responding to frustration; an example of this is provided in Bettelheim's account of the persistent submission to authority of the German middle class as a factor interfering with a constructive adjustment to a concentration camp (3, pp. 425–426). Finally, the role of situational factors may be illustrated indirectly by Bettelheim's point that the typical adjustment of various categories of prisoner differed because of the different significance of imprisonment for them in view of their backgrounds (3, pp. 424–429).

A somewhat different kind of case in which the question of pervasive effects on quality of performance arises, is the case in which a person's behavior is simply noticed to be conspicuously high or low in general quality, and there is the possibility that this condition is in part a product of psychodynamics. Psychotics, some neurotics, and some apparently feebleminded persons may be seen as persons with a generally low quality of response which seems to result from dynamic processes of adjustment to life situations. "Frustration" is certainly no adequate label—if there be one —for the great variety of situations to which is attributed an important role in these processes. Yet frustration certainly is one of the variables relevant to understanding these processes. The opposite extreme, of the genius with a generally very high quality of performance, has been little studied from a psychodynamic point of view, so that we can hardly venture a judgment about the probable role of frustration here.

In cases of neurosis or psychosis the applicability of our general approach is very aptly illustrated by certain aspects of the recent book by Dollard and Miller (9). In their interpretation of such pervasively maladjustive behavior, they stress the role of a person's previously established habits of response to strong emotion or to specific drives. In particular, habits which prevent the correct labeling of an emotion or drive tend to reduce the quality of behavior by eliminating the potentialities for fine discrim-

ination and appropriate generalization which may be brought into a behavior sequence by the use of correct labels. An account of neurotic or psychotic behavior in such terms tries to explain much more than the patient's reaction to frustration; it tries to explain also, for example, his reactions to goal attainment. But the point relevant here is that, if this is a useful explanation of neurotic behavior in general, it is also specifically an explanation of why neurotics may tend to make responses of poor quality in frustrating situations.

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AGNOSIA IN ANIMAL AND MAN [1]

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To a psychologist from Mars, it might seem paradoxical that for us the study of integrative mechanisms of behavior has become almost exclusively the province of the animal psychologist. Complex processes, such as learning, problem solving, and concept formation, are studied primarily in infrahuman animals, which, whatever their virtues, are not noteworthy for these abilities. This displacement of the human subject by the rat or monkey appears to rest on two related articles of faith: First, that the taxonomy of cognition is mature enough to allow study of the interrelation of established variables in the simpler and better-controlled situations that the use of animals permits. Second, that the laws and mechanisms which are uncovered in one species will apply equally well to others, and that a complete "phylogeny of behavior" merely awaits appropriate adjustment of the constants in some fundamental equations.

No doubt these beliefs have considerable heuristic value, but since, whether we will or no, generalizations from animal studies will be made to man, the validity and adequacy of such extrapolations should be tested empirically. Perhaps there are certain questions, such as localization of function in the brain, which can never be answered for man with the same certainty as can be attained for animals. Yet, conversely, perhaps there are questions which are logically prior to anatomico-functional correlation, to which experimentation on man alone is likely to provide answers. Cognitive processes, as far as we know, reach their highest development in the human species. Language exists only in rudimentary form, if at all, in other animals. Because of his capacities, injury or disease may produce in man disorders such as the agnosias, apraxias, and aphasias, which offer an extraordinary opportunity for study of the structure of mind.

The opportunities presented by lesion of the nervous system in man are not now being exploited by any large or organized group of psychologists. Most physiological psychologists scorn clinical material, because exact knowledge of the locus of lesion in human cases is usually lacking. It is ironic that such an attitude may exist even among experimenters who fail to provide histological verification of the ablations they report in animals. It should be apparent that, if the aim of an experiment is localization of the lesion necessary and sufficient to produce a given symptom, then neither man nor animal in which locus is incompletely specified is a suitable experimental preparation. However, symptoms of brain damage raise a question other than localization, which is generally overlooked, namely, what is the nature of the alteration in behavior? The answer to this purely psychological question is relevant to theories of normal psychological organization. It

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is also an indispensable prerequisite to the adequate design and interpretation of localization studies. If the aim of an experiment is to describe and categorize precisely the nature of the behavioral change, then man is an eminently suitable experimental subject, and the fact that we cannot localize the causative lesion except grossly need not deter us from such an attempt. As Lashley (24) puts it, "The study of the effects of brain injury should serve as alternative to factor analysis, partialling out elementary functions or abilities which may vary independently."

What professional groups are in contact with material suitable for such studies, and how have they used it? Clinical psychologists are one such group, but so far have shown little interest in these problems. Among clinicians, evidence for the influence of the psyche on the soma is greeted enthusiastically (if not always critically), yet the complementary question of the influence of the soma on the psyche is neglected. The content of research in clinical psychology is typically the emotional "dynamisms" of the person; the usual aim is psychiatric diagnosis or therapy. Man as a rational animal is studied mainly by means of a few intelligence scales of an over-all character, the problem being generally conceived as how best to find a number which will represent the total intellectual attainment of the subject. There is little current research directed toward analysis of intelligence, and even less which makes use, in such analysis, of the leads offered by dissolution of complex nervous function.

Another group with opportunity to study the process of psychological disorganization consequent to damage of the nervous system is of course physicians, and some outstanding contributions have been made by a few neurologists, notably Hughlings Jackson, Head, and Goldstein. Their studies were not aimed at the discovery of diagnostic rules of thumb, but rather at the resolution of the factors underlying alterations of behavior or experience. All of them were greatly influenced by contemporary psychology, and it would be profitable to re-examine their notions in the light of present-day psychological theory. Their concepts require reformulation in operational terms, and their conclusions should be subjected to empirical test, using modern logical and statistical methods of inference.

Certain ideas and controversies, which have long been debated among neurologists, offer virgin territory for controlled experimentation. Jackson (17) held that the "organ of mind" is "only the most complex part of . . . a sensorimotor machine," that is to say, that even the highest centers represent parts of the body in terms of patterns of sensation and movement. This principle raises questions concerning the relation of sensation and movement to conceptual and symbolic processes, and to appropriate conduct. One of the more important of these questions to psychology is that of agnosia. Is it possible, for example, to lose visual recognition of objects in the absence of blindness or dementia?

Attempts have been made to attack the problem of agnosia in animals, with equivocal results. It is not clear, of course, that the syndrome could occur, or be recognized as such, in infrahuman species. Some first steps have been made, however, toward elucidating the nature of the relatively isolated defects in differential reactions which can be produced in animals by cerebral lesions. I should like to review briefly the outcome of these studies, and then discuss the reasons why further experimentation on man is likely to be necessary in order to resolve the issues.

According to traditional conceptions

of localization of function within the cortex, lesions of the "visuopsychic" band surrounding the visual sensory area should produce visual agnosia. Ablations of this region in monkeys have been studied independently by a number of investigators (1, 7, 8, 11, 24). Since this lesion involves risk of damage to the underlying optic radiations, unambiguous interpretation of any consequent abnormality demands histological demonstration of the intactness of the projection system. In the studies which have controlled this factor, no defects in visual function have been found, even though some of the problems required complex visual integrations, and were difficult enough to be near the limit of the monkey's capacity. Thus the ablation which on a priori grounds should be most likely to produce visual agnosia has failed to do so. I have attempted to produce tactile agnosia by restricted lesions of the "somatopsychic" area, and have likewise failed (5).

Although lesions of the areas supposedly concerned with sense-related ideas do not produce behavior resembling agnosia, ablations of cortex more remote from sensory areas may be followed by gross defects in discriminative behavior. Klüver and Bucy (19, 20) found that bilateral temporal lobectomy in monkeys produces undifferential approach responses to a wide variety of objects, regardless of whether these stimuli elicited approach, avoidance, or indifference in the normal animal. The syndrome also includes the tendency to examine all objects by mouth, acceptance of foods which are normally repellent, and greatly increased sexual activity.

Since this aggregation of abnormalities is difficult to derive from one which is fundamental, attempts have been made to dissociate the various defects by modification of the lesion. The temporal lobe includes two structurally distinct parts—paleocortex and neocortex. On the other hand, the temporal neocortex is structurally indistinguishable from that of the parietal and preoccipital regions (25). Chow, Pribram, and I (6) therefore considered that a reasonable preliminary approach to analysis of the syndrome might be to compare the effect of removing the parieto-temporo-preoccipital neocortex (excluding primary sensory areas) with the effects following traditional temporal or parietal lobectomies. The parieto-temporo-preoccipital operation was consistently followed by defects in visual discriminative learning, as measured by performances on formal habits of differential reaction to colors and patterns. All animals showed impairment on these tasks, although only half the animals showed behavior suggestive of the symptom which defines agnosia in man, that is to say, lack of customary reaction toward familiar objects.

Our rather extensive program of pre- and postoperative training and tests, and postmortem histological studies, allowed us to draw certain conclusions about the nature of the deficits on the formal visual discriminative tasks. In the first place, the deficits were something more than amnesias. The animals not only lost the habits to which they had been trained before operation, but had abnormal difficulty in reacquiring them. Second, the deficits were not caused by primary sensory impairment. Although some of the animals had primary visual defects consequent to invasion of the optic radiations, not all the animals suffered such damage. Impairment of visual discriminative habits was present in some animals in which the primary afferent system was completely intact, and in which there were no demonstrable scotomata or losses in acuity. Third, the deficits were specific to vision. Analogous somatosensory discriminative habits were much less

consistently and profoundly affected. Fourth, the deficits in visual discriminative learning were dissociable from gross spatial disorientation, from hypersexuality, and from aberrations of feeding behavior.

One of the questions raised by these results is why some of the animals showed impairment of object recognition and of orientation in space, and others did not. In our animals these abnormalities were present only in those individuals which had primary visual defects in addition to impairment of the ability required by the formal visual discriminative tasks. We know from the results of other studies, however, that scotomata and losses of acuity are in themselves insufficient to cause the symptoms of agnosia. Therefore, we hypothesized that two factors are necessary for the production of agnosia: One factor is related to damage to the higher levels of the sensory projection system, the other to lesion of "associative" cortex. This hypothesis recalls von Monakow's (26) view that permanent visual agnosia in man results only from combined lesion of both primary visual and other cortical areas. Additional data in support of such a two-factor theory might help to reconcile conflicting opinions concerning the basis of agnosia.

How to characterize either of these factors in the language of psychology, if they can be adequately described in such terms, is at present unknown, and is unlikely to be discovered from further animal experimentation. The first factor might be called "sensory," because it is the result of damage to the thalamocortical afferent system; but to what specific inabilities do we refer when we say "sensory defect"? We know that circumscribed damage to the striate cortex produces regional blindness, but we also know that this defect is not equivalent to that caused by peripheral damage (4, 23, 30).

The second factor is defined anatomically by lesion of "associative" cortex, and behaviorally by isolated impairment of visual discriminative learning. But how can we characterize in psychological terms a kind of deficit which is not visual, and yet is specific to vision? For example, normal monkeys learn color or brightness discrimination problems readily. A monkey with lesion of the temporal lobes, however, may be unable to learn to choose green in a red-green discrimination problem, although he immediately demonstrates a preference for red, and in that sense can discriminate. Monkeys with such lesions have abnormal difficulty in acquiring formal habits of discrimination between black and white, although they can easily distinguish a fine black thread against a black background.

In attempting to discover what the nature of this deficit may be, at least two major possibilities must be considered. The first is that what is lost is the capacity to use visual sense-data, which are themselves normal, as the basis of an acquired discriminative reaction; the second is that the sense-data themselves are altered in some subtle way.

Let us examine the first possibility that the deficit is an impairment of the capacity to use normal sense-data of a particular modality in acquired discriminative reactions. We might assume that each synaptic level in a sensory system represents a point of conjunction between the incoming sensory impulses and some class of reactions, and that the temporal neocortex is the point at which visual impulses influence most directly the processes on which the abilities required by formal discriminative tasks are based. These processes could still be activated by other modes of sensory stimulation, and consequently it could be shown, as in fact we did show, that discriminative learning in an-

other modality was not impaired similarly. With regard to various visual functions, the hypothesis predicts that whether they are lost or retained does not depend on the theoretical level of complexity of the function, but rather on the manner in which each is tested. It is the testing methods which are dissociable instead of the capacities supposedly tested. Much of the evidence from cases of agnosia in man points to the same conclusion. For example, recognition of an object is possible in one set of circumstances but not in another. Apraxia is similar in nature, as illustrated by Jackson's famous example of the patient who could not protrude his tongue on command, but who could lick crumbs from his lips (18). Both agnosia and apraxia can thus be conceived as a narrowing of the range of equivalence: in agnosia, restriction of response equivalence, and in apraxia, restriction of stimulus equivalence. I cannot emphasize too much, however, that there exists no adequate psychological formulation of why the responses, or the stimuli, are no longer equivalent.

The second possibility is that the isolated defects in discriminative reactions found in monkeys, and the symptoms of agnosia in man, are the result of a subtle alteration of the sense-data themselves. Head and Holmes (16) described the characteristic changes in tactile sensation produced by a cortical lesion as inconstancy of threshold, rapid local fatigue, hallucinations of touch, and abnormal persistence of sensations. Stein (29), working with cases of sensory involvement from central nervous lesion, showed that repetitive testing with von Frey hairs causes relatively great elevation of tactile thresholds. This may be true even of parts where the threshold appears normal to ordinary test. He suggests that difficulties in discrimination may be caused by this lability, that is to say, the first stimulus perceived may raise the threshold sufficiently so that a subsequent stimulus is not differentiable. Using Stein's methods, Cohen (9) examined several patients who showed defects of recognition with little or no sensory impairment, and found in all of them marked lability of thresholds and persistent after-sensations. The alterations of sensation which are revealed by the method of double simultaneous stimulation (2, 3, 10) may also be relevant to the problem of impairment of recognition of complex objects. If one stimulus "extinguishes" or "obscures" the perception of another, or displaces its subjective position, the resultant impression might be sufficiently different from normal perception to make recognition impossible. Assuming that such aberrations of the sensory process underlie the impairment of discriminative learning found in monkeys after lesions of "associative" cortex, a new conception of the function of these areas emerges. The traditional view is that impulses from the primary sensory areas are organized and patterned in "associative" cortex; instead, it may be that impulses from "associative" cortex play back upon the primary sensory areas, thereby modulating and stabilizing the sensory process.

It is apparent from this cursory survey of experiment and speculation concerning agnosia that the psychological problems presented by this syndrome are still unsolved. The animal experiments suggest the form of the solution, but they tell us little about the substance of the postulated factors. Even if we had the answer for the monkey, we still would not necessarily have it for man. A truly comparative attitude should prevent us from hasty generalization from one species to another even within the primate series. Whether the formulation developed in animal work is adequate for analysis of agnostic symptoms in man, and if so, how the

factors can be described in psychological terms, are empirical questions.

In collaboration with H. L. Teuber, S. Weinstein, and L. Ghent, I am attempting to develop empirical categories of somatosensory function in terms of symptom-complexes after brain lesions in man. To this end, men with proven loss of brain substance are given a series of tests of somatosensory and presumably related functions; all tests yield quantitative scores. The resulting profile is analyzed for relationships among symptoms, and compared to analogous profiles derived from the control groups, in order to clarify the nature of the alteration in the somesthetic process.

Population studied: The subjects of this study are drawn from the case load of the Psychophysiological Laboratory of New York University, gathered by H. L. Teuber and M. B. Bender for study of the after-effects of penetrating brain injury. These men are veterans, primarily of World War II, who have sustained combat injuries (gunshot wounds) of the brain or of peripheral nerves. They are ambulatory and report to the laboratory voluntarily for individual testing sessions. Most of these cases represent presumably stable lesions, in which further restitution of function, during the course of our survey, is unlikely to occur. The study of such stationary cases has obvious advantages for determination of the effects of loss of tissue per se. However, in order to extend the generality of our findings, we have recently begun testing also a group of cases in which benign tumors or encapsulated abscesses have been radically removed by the neurosurgeon.

Subgroups formed: The experimental group has brain injury and consequent somatosensory symptoms (as defined by the sensory tests in Group A, described below). There are three control groups, also made up of wounded veterans: one group with brain injury but without somatosensory symptoms; one group without brain injury but with somatosensory symptoms caused by peripheral nerve lesion; and one group without brain injury, and also without peripheral nerve lesion affecting the parts tested. Each group includes about thirty individuals.

Methods of obtaining data: The test program includes: (A) an evaluation by psychophysical methods of basic somatosensory functions, such as tactile sensitivity, cutaneous resolution of two points, and point localization. Measures of strength and coordination, and ratings of ability to perceive direction of passive movement of a joint, of tone, and of reflexes, supplement these data. (B) A quantitative study of aspects of the sensory process which are usually neglected, such as latency of sensation, adaptation time, duration of after-sensations, the effect on thresholds of prolonged testing, and of concurrent stimulation of other body parts. (C) Tests of discriminative ability, among forms, patterns, sizes, weights, textures, and grades of roughness. In these tests, the subject is given a series of models, and is asked to find duplicates from among an array. The procedure is carried out both unilaterally (the same hand feels the model and finds its duplicate) and bilaterally (one hand feels the model and the other hand finds the duplicate). (D) A comparison of the ability to solve problems and to derive concepts when the elements are perceived tactually, or visually. These tests include matching-from-sample, conditional matching, conditional reaction, abstraction, and sorting tests (**12**, **13**, **14**, **15**, **21**, **22**, **27**, **28**). There is a visual form and a tactual form of each test. They will be described in detail in forthcoming publications; briefly, in each test the subject must discover a principle in order to respond correctly. The procedures are designed to minimize the effect of aphasic difficulties; they involve choice-reactions in a series of trials, approximating methods used in the animal laboratory. (E) Tests of orientation in external space and with respect to the body scheme. The subject is required to walk through complicated paths indicated on maps, perceived visually or tactually. Spatial sense, regarding the body, is tested by requiring the subject to touch in succession a series of points on the body

surface indicated on diagrams of front and back views of the body.

Methods of analyzing data: For each test, the performances of the groups are compared, and the differences evaluated statistically. The relationships among the various tests are determined separately for each group, by correlational techniques, in order to disclose patterns of normal and pathological function. Disparities among these group patterns will be examined for the purpose of arriving at an adequate description of the organization of somatic sensation at the cerebral level.

With this program, we hope to gain a clearer understanding of how somatic sensation and presumably related abilities are altered by central and peripheral lesion. We hope to find out if isolated disturbances such as those we produced in monkey occur in man, and if this is the case, what these disturbances mean. To what extent are certain somatosensory functions lost together, or preserved together, following cerebral lesion? Can discriminative function be disturbed independently of *any* alteration of the sensory process? Can there be disparate levels of ability to solve problems, dependent on the sense through which the relevant information is received and independent of sensory impairment? Can deficits in spatial "intelligence" be related to somatosensory or to visual defect? Can such deficits be specific to the body, or specific to external space? What systematic differences exist between normal patterns of sensory function and those found after lesions of the central, or peripheral nervous system? More specifically, are there differences in regional gradients of sensitivity, in the relation among various measures of sensory capacity, in temporal and spatial spread of effect of stimuli? The associations and dissociations of symptoms, which can be brought to light only by intensive study of the individual case, should contribute to our knowledge both of the basis of agnosia and of the normal gnostic process. The methods of classical experimental psychology and of the animal laboratory, when applied to the study of clinical material, may on the one hand yield more valid and reliable data than have hitherto been obtained with pathological cases, and on the other hand, richer and more understandable results than have been obtained with animals.

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